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JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION

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HOW TO STUDY THE SOCIAL ASPECTS OF THE DEPRESSION*

By Edwin B. Wilson
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Distinctions and differences.—The use of the definite article "the" before the singular noun "depression" instead of the indefinite article "a" or instead of the plural noun means that the discussion is limited to the recent unlamented unpleasantness of 1930 to 1935; it means that we are not interested in the social aspects of depressions, we are to discuss the way to make a case history study of a single though infinitely complex, instance. Pushed to the extreme this is an attitude often found in the medical clinician who says that no two cases are ever alike or in the historian who says that history never repeats itself and that analogies are misleading. There is much to be commended in this point of view. It is perhaps better justified in the sociologist who does not have the length of life to see several major depressions than in the clinician who does see a large number of cases of the same disease. Furthermore, even if the sociologist should develop, as G. B. Shaw urges for statesmen, in *Back to Methuselah*, a normal life span of 300 years, he might well contend that technology so changed in the long intervals from depression to depression that he must admit the validity of the historians' contention that the instances were indeed non-comparable.

The insertion of the article "the" before "social aspects" instead of its omission, implies on the other hand that we are interested in the whole complex of social aspects of the depression. This complicates our problem by introducing the "total situation" with all its unsolved methodological difficulties. Finally, whether the adjective "social" is intended to restrict or to amplify the noun "aspects," and in what way, is not clear. In the Foreword to the thirteen *Studies on the Social Aspects of the Depression* the monographs are described as a series designed "to stimulate the study of depression effects on various social institutions." This is, at least in form, a more specific statement. It implies that we are interested in some degree of generalization,

* The opening paper at a joint session of the American Statistical Association and the American Sociological Society, Atlantic City, New Jersey, December 30, 1937. The subject of discussion was the experience of the authors of a series of monographs on "Social Aspects of the Depression." (See p. 614.)

for "depression effects" means just that as contrasted with "aspects of the depression," and "various social institutions" is by no means synonymous with "the social aspects." We have a greater emphasis on causative relationships as contrasted with descriptive accounts and this introduces the very serious methodological point of the disentanglement of causes in a complex field.

I do not mean to imply any confusion on the part of writers or editors in the contrast I have drawn between the phrases "the social aspects of the depression" and "depression effects on various social institutions." It is not unlikely that one good initial step in the stimulation of the study of depression effects on various social institutions is a series of monographs on the social aspects of the depression. I wish merely to point out that there is a distinction and that I may have something to say about each of the problems thus distinguished.

The literary attack.—If we are interested chiefly in the social aspects of the depression as a case study we may perhaps best proceed as the Lynds do in *Middletown in Transition*. They visit the place, talk with some large number of various persons, and compose a breezy and highly readable book. This is not science, any more than the *Charge of the Light Brigade* was war; but both have a magnificence which increases the *élan* of a wide variety of persons, whether poet or peasant, whether professional or proletarian. And it is a study. Indeed it would have been a study even had the Lynds stayed at home to write it. I should not be surprised if time should prove that the best single study of the social aspects of the depression were to be found in some inspired novel or play. Let none say that such are not studies simply because the authors have not the doctorate and do know how to write. Until the social field is better surveyed and better schematized than at present one may find as good politics in Aristophanes as in Plato, as good sociology in Dickens as in Spencer.

Even in the economic field something may be garnered from highly inspired literature. You are familiar with the epic put together by an ancient unknown writer recounting the deeds of a dreamer who was sold down into Egypt and who had such insight into the ways of God and of men that he foretold a long cycle of fat and of lean years so effectively that he was advanced to the position of Secretary of Agriculture to one of the Pharaohs, whereupon he developed plans for an ever normal granary that must hold the admiration of our own Secretary thereof and furthermore carried out the plan with a completeness which may well be envied. He did not plough under the grain of the good years but stimulated its production and laid upon all farmers a special levy of 20 per cent of their production which he stored in ware-

houses distributed throughout the land until they bulged with the excess of the fat years. And when the lean years came he fed the people of his own adopted country and of others. He did not feed them for nothing; he made them pay. First he got all their money, then all their horses and flocks and herds and asses, and finally all their fields so that the land became the Pharaoh's. After this he resettled the people in the cities, perhaps to take their labor for public works in return for their food. So when at length the last lean year drew to its close and he came to issue them seed that they might sow the land which was now the Pharaoh's he laid upon the grateful if impoverished people the condition that one fifth of their harvests should forever belong to the Pharaoh—that was one social consequence of that depression, the people had become bond servants of Pharaoh. From some remarks I have recently seen attributed to another and different Joseph I should judge that Professor Davis is not of the group who is sure history does not repeat itself. At any rate one might inquire whether a temporary economic reliance of a people upon government usually results in a longer loss of liberty or even in a loss of a desire for freedom.

The informational attack.—I would not imply that all writing is to be extolled as worthy of serious attention. I said inspired writing, albeit I should have difficulty in formulating precise criteria of inspiration. You will recall that of the Athenians at the time when St. Paul was led into the Areopagus it was written (Acts: 17, 21): "For all the Athenians and strangers which were there spent their time in nothing else but either to tell or to hear some new thing." On the fourth day of these meetings at Atlantic City you surely will not begrudge me that quotation. I could say as much were I with the American Association for the Advancement of Science at Indianapolis. Of the many contributions there or here few are at this time contributions to science; they are too new. Science consists of the old things, of things old enough to have become accepted into a corpus of demonstrated knowledge and available as valid working hypotheses for further coherent developments. It is as difficult for the new to enter into science as it is for the *nouveaux riches* to enter into the Kingdom of Heaven. I would not be interpreted as slandering the Athenians or moderns as mere gossipers. The search for the new, the wonder at the unknown, the interest in the half known are such basic drives that they may well be classed with the "residus." Without them there would be no science and no revision of science. As close students we tend to overelaborate detail and to overexpand systematization, and thus to retreat from reality. It is the strange new things which we may tell or hear that serve as *apéritifs* to study and as *aperients* for overstudy.

This may all seem quite frivolous. It appears to me, however, that possibly there may inhere in some such liberal view as I have adumbrated a really meritorious suggestion as to the different methodologies which may be appropriate to the studies which attempt to understand the "total situation" as contrasted with those which attempt to add to our strictly scientific knowledge. Can a strictly scientific characterization of a "total situation" be expected to be attainable? May we not have forever to be content with an understanding of the sort the artist has and may not the best representation of a total situation be some sort of picture, more or less subjective, with large tolerances as to detail and even with certain poetic licenses to distort detail if it so be that the picture as a whole will thereby give a truer impression of the "total situation?" May we not have here some valid analogy with *diagnosis* in medicine. It is, I suppose, a well accredited fact that the good physician often makes a good diagnosis before knowing why or how he makes it. Diagnosis may not be so much a scientific as an artistic procedure, but if by the accumulation of experience one can arrive with reasonable correctness at a result, i.e., if different persons arrive independently with reasonable unanimity at an agreement as to the essentials of a total situation, may not that consensus be itself as good a basis as any other for the validity of the result? The subject is too much to handle here,—I merely wish to raise it. Should not a number of Lynds do *Middletown* and the Lynds do a number of *Middletowns*? A physician sees numerous cases and consultations of physicians on a case are not unknown.

The handbook attack.—After dwelling so long upon these literary or even gossiping methods which are so dear—and peradventure so necessary—to the social sciences that one may not proceed without paying them their due homage, I may suggest another attack on how to study the social aspects of the depression. If I would study the epidemiology of measles, as I am doing in such odd moments as are left me after satisfying the most pressing importunities of persons who would dissipiate my energies over all sorts of interests personal to them —such as this program—if I would study anything, my first reaction born of some considerable experience in a variety of fields is to consult the best handbook or encyclopedia upon the subject. So in an effort to get a flying start on the serious part of my paper for this symposium I instinctively reached for the index of the *Encyclopedia of Social Sciences* by Seligman, the Rockefeller Foundation, Alvin Johnson, *et aliis*. Therein I found in small type "Depression-Economic —see *Crises*. This gave me a bit of a depression. I had innocently supposed that depressions were sufficiently striking phenomena to be

worth a factual treatment all their own. Moreover, if the topic of this conference were depression as a cause of social effects I did not see much hope in a discussion of depressions as the effect of economic log-jams. I felt as though, being interested in anemias, I had turned to a medical encyclopedia and found the direction *see hemorrhage*—that is one way to become anemic.

However, there was nothing to do but follow the lead. So I looked up *Crises*. There was reference to the chief article *Crises* by Jean Lescure in 7 columns. Here was found plenty about Crises but little about depression. It was stated that crises have become of diminished acuteness and finally that "for the term crises one may henceforth substitute that of depression." I was of course glad of the implication that the late depression was less acute than former ones and thus lets us look forward with hope toward the next, but I wondered why if we should replace the term crises by that of depression, the Encyclopedia had not taken the hint. Next there was a reference to the second column of a 28-column article on *Business Cycles* by W. C. Mitchell which is naturally a very fine treatment of business cycles but quite appropriately not particularly informing about depressions for my purposes. Then there followed several references to economic theories of crises with special emphasis apparently on those of Marx which were not very helpful to me as I was not looking for opinions but for facts. There was a reference to Clement Juglar "who in 1862 first demonstrated the rhythmic cyclical character of economic activities" and one to Jean Charles Léonard Simonde de Sismondi "who in 1819 emphasized the fact that general crises are . . . an inevitable periodic concomitant of the prevailing economic structure." Clement Juglar or Simonde de Sismondi, take your choice, but for light on depressions you might as well be referred to Simone Simon—she might really help.

A stubborn fact.—So I gave up the Encyclopedia of the Social Sciences as a useless and contradictory statement of opinions without factual backing. What to do next? My confidence in encyclopedias, (of mathematics, physics, engineering, medicine) which had become so firmly established in my various efforts at study was now so rudely upset by my present experience that I suffered one of those frustrations you hear about. In this moony state of bewilderment I chanced to think of an annoying fact of my wife's experience which according to her statement has a certain degree of general validity, and that fact was that with anywhere from 2,000,000 to 12,000,000 persons unemployed it has been particularly difficult to get and keep a maid. Is this a social aspect of the depression? The employment offices and all her friends answer that it is. Then I recalled a talk early in the depression with

my old friend John Candler Cobb who said that he was long since retired from business and was not *au courant* with affairs but that he somehow did feel (note the diagnosis and not the scientific inference) that prosperity was not just around the corner but that we were in for a real depression such as he recalled in the 1890's and even in the 1870's. He said that in those days, as they got worse and worse, people would come looking for work, begging for work, just for their keep, and that perchance this depression might relieve the serious problem of domestic servants. Did it? And if it didn't, but if on the contrary the problem grew worse, was that a depression effect on a social institution or was it the effect of a governmental policy itself arising out of the depression? And next, is a social aspect or effect of a governmental policy arising in a time of depression a social aspect or effect of the depression?

The statistical issue.—The answer to the questions just raised throws us into matters of methodology. It involves the conceptual schematization which we adopt. If the aspects $a_1, a_2, a_3, \cdots$ are functions of certain variables $v_1, v_2, v_3, \cdots$ which are themselves functions of certain depression indices $d_1, d_2, d_3, \cdots$ as

$$a_i = f_i(v_j), \qquad v_j = k_j(d_k),$$
$$i = 1, 2, \cdots, p, \; j = 1, 2, \cdots, q, \; k = 1, 2, \cdots, r$$

these variables being partly known and partly unknown (or maybe not even suspected) and the known ones being partly measured and partly not measured, shall we say not only that the a's are functions of the v's and that the v's are functions of the d's but also that the a's are functions of the d's—in other words may we substitute and get

$$a_i = f_i(h_j(d_k)) = g_i(d_k), \qquad i = 1, 2, \cdots, p, \; k = 1, 2, \cdots, r$$

thus eliminating the v's? This is ordinarily possible and often done in those realms of science like mathematical physics (including mathematical astronomy) which are often taken as the prototypes of all scientific thinking. We should not overlook the fact that such formulation is not stressed and may be inappropriate in systematic botany or zoology, in organic chemistry, or in medicine, and we may recall that Marshall emphasized the opinion that biology offered a far fairer analogy than physics to economics. In those cases where there is real variation in the data and where resort to statistical procedures seems inevitable we do not have the possibility of elimination by substitution. Even in the simplest case of only one variable we have linear regression equations:

$$a = lv + m, \quad v = pd + q \quad \text{but not} \quad a = lpd + lq + m.$$

The reason for this is that the left hand side of a regression equation contains not the variable usually written there but its mean value in an array. We should write

$$A = lv + m, \qquad V = pd + q$$

where A, V designate array means of a and v, and in that form one would not be tempted to try to substitute. It is of course possible to write $A = rd + s$ but r cannot be calculated from l, m, p and q, it must be calculated independently.

We are entirely at liberty to adopt a conceptual scheme if we choose which relates the a's to the d's or one which relates the a's to the v's either with or without one which relates the v's to the d's. It does however make a great difference in our final product according to which scheme we do adopt; it also makes a difference in our statistical procedures in case we try to study the matter of depression effects on social institutions by the statistical method. We are presumably all aware of this fact; if we are not, we may study Sewall Wright's papers on his method of path coefficients which have the great merit of making it apparent how seriously the results depend on the schematization adopted. I would not imply that the state of most problems regarding social aspects of the depression is as yet such as to make profitable the attempt to apply to them Wright's method of path coefficients; I wish merely to suggest that until a problem reaches a state where such analysis is valuable we may do well to bear in mind the fact that the problem is not yet in that state.

Envoi.—The very self-sacrificing persons who were good enough to devote a considerable fraction of their energies and intelligences for a long period of time to the preparation of the thirteen monographs on *Social Aspects of the Depression* found, as they frankly admitted, many difficulties which they tried in many ways to turn and which in some cases they did turn with a greater ingenuity than could have been expected even of them. And when they could not turn the difficulties they were often successful in saying how information might be collected which should make it possible to overcome them. In this way these little volumes become at once a great source of indicated desiderata in our data and a no lesser source of stated problems which should be attacked. We may therefore congratulate the authors and ourselves on their accomplishments not alone with respect to the information they have brought together but more especially for the suggestions for further work. Certainly today one of the best methods of studying the social aspects of the depression is to attack some of these their many suggestions.

I do not dare to close without saying that if we are intelligently to discuss depression effects on social institutions we should not overlook the comparative study of a large number of depressions widely separated in space and in time. I very much wish I knew of a good finding list of depressions. True, there cannot be many besides the latest which can be studied satisfactorily on a quantitative statistical basis, but the first step in any comparative study is "systematic," it is to pick out the qualitative characteristics which seem to be of some importance so that we may make a beginning of a hopeful conceptual schematization through the application of such general laws of logic as Stuart Mill's canons of agreement and difference. History is as indispensable a laboratory to the social scientist as is the present-day field.

THE χ^2-TEST OF SIGNIFICANCE*

By Thornton C. Fry
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1. THE BASIS OF THE PROBLEM

The task which has been set me by your program committee is that of explaining the mathematical derivation of Pearson's χ^2-test, thereby setting the stage for the gentlemen who are to follow me, upon whom devolves the task of making pertinent comments as to the statistical usefulness and limitations of this test. This arrangement obviously relieves me of the necessity of being at all profound or of saying anything new, and I shall make no attempt to do either. Instead, I shall try to present a rather involved mathematical argument as simply as I can and at the same time to bring out the various assumptions which underly it and the numerical approximations with which it is beset.

To begin with, of course, we must know what the problem is which we are trying to solve, which means that we must agree upon the uses to which the test is to be put. Now it happens that the situations to which the χ^2-test is applied are all of a very simple type. They are cases where two things exist: a set of experimental (I like the word better than statistical) data and a working hypothesis regarding certain probabilities associated with the experiment. From these hypotheses we can compute what we would expect from our experiment on the average. The data will seldom agree exactly with these expectations, however, and hence the question arises in any particular case as to whether the observed discrepancies can reasonably be regarded as chance variations, or whether they are so remarkable as to throw serious doubt on the adequacy of our working hypotheses.

For example, the experiment may have been a simple coin-tossing, and the data may be the number of heads and tails that appear. A working hypothesis may be that the coin is true so that heads and tails are equally likely. The question then is: In the light of the experiment, are we still warranted in retaining this hypothesis, or should we conclude that the coin is almost surely biased?

Or we may have as our data the vital statistics of a large city showing the age at death of a great number of people, and as our working hypothesis a certain mortality curve. The question to be answered is then: Does the assumed curve represent the data fairly, or should we conclude that it is not appropriate to the population of this particular city?

*A paper presented at the Ninety-ninth Annual Meeting of the American Statistical Association, Atlantic City, New Jersey, December 27, 1937.

Or we may have before us the record of a card calling experiment of the sort which is being carried on by Professor Rhine at Duke University, and as our working hypothesis the assumption that this record is the result of accidental chance guesses only. The question before us is then whether the record is consistent with this hypothesis or whether we are forced to seek another explanation.

Now there are four pertinent comments regarding questions of this sort which I would like to make before even attempting to discuss how they would be answered.

The first is that each such question refers to a *single* set of hypotheses. We are not viewing the data in the light of several possible hypotheses simultaneously and asking whether, in the light of all the information we possess, one is more strongly supported than another. We may, of course, take up a number of such hypotheses in succession and find in the end that our tests of significance mixed with a liberal amount of common sense enable us to rank them relatively to each other. But so far as the purely statistical procedure is concerned, this amounts to solving a number of separate problems each dealing with one, and only one, hypothesis. In this respect we shall find that the χ^2-test is in sharp contrast with Bayes' Theorem which does view the various possible hypotheses collectively and rates them relatively to one another, but which makes the essential requirement that every possible hypothesis be accounted for.[1]

The second remark is a sort of corollary of the first: it is that though a test of significance if it comes out negatively may have the practical result of *discrediting* a particular hypothesis, a positive result would not *prove* it. It would still remain merely an acceptable working hypothesis. This statement is not at variance with the practice so common in the sciences of regarding a working hypothesis which stands up under repeated comparison with fact as proved, for such a position is taken only as a consequence of many tests collectively, and usually because of the failure of rival hypotheses to meet these tests.

In the third place, even with the same data and the same hypotheses to begin with, problems of this sort can frequently be formulated in different guises, in some of which the discrepancies may be rated as significant and in others not. This does not mean, however, that any of the answers is wrong, but merely that in certain cases we have used the available information more effectively than in others. For example, the vital statistics which I mentioned a while ago could be classified in five-year intervals and compared with the expectations as deduced

[1] An elementary discussion of Bayes' Theorem emphasizing these points will be found in *Scripta Mathematica*, II (1934), pp. 205–221, under the title "A Mathematical Theory of Rational Inference."

from the areas under corresponding segments of either of the two curves of Figure 1. They might then show conclusively that the dotted curve was not acceptable, but still leave the other as a satisfactory working hypothesis. If, however, the data were classified into only two groups—those below age 50 and those above—both curves would stand up equally well, since they both predict the same proportion of deaths in these two groups.

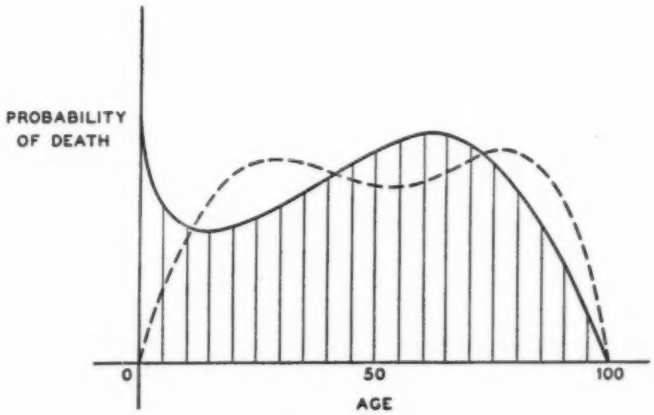

FIGURE 1

In this case a fairly fine-grained classification is desirable in order to extract the maximum possible amount of information from the data. By and large, this is the usual situation. It is therefore an important, though unfortunate, aspect of the χ^2-test, that it establishes limits beyond which this fine-grained structure must not be carried.[2]

The fourth and final remark is this: that the very essence of our argument is to inquire to what extent the discrepancies between expectation and observation might reasonably be accounted for by *chance*. If, then, with the data already before us, we deliberately rig up a set of hypotheses so as to disagree violently with the data, we will learn nothing important from applying a test of significance and concluding that the discrepancies could hardly have been due to chance. We already know they were not. Conversely, if we rig the hypotheses into close agreement with certain aspects of the data, we will learn nothing important from observing that the agreement in these respects *might* have happened accidentally; we already know it did not.

Now this observation is very important because it is customary to rig our hypotheses to fit the data in certain respects. We do it whenever

[2] See in this connection the paper, "The Multinomial Solid and the Chi Test," by B. H. Camp, *Transactions of the American Mathematical Society*, 31 (1929), pp. 133–144; and 44 (1938), p. 151.

we determine the arbitrary constants of a distribution function from certain properties of the data. Obviously then, our mathematical treatment must be phrased in a sufficiently general fashion to answer the question whether an experiment which has been forced to agree with the hypotheses in such particulars could be expected by chance alone to exhibit the discrepancies which remain.

We may as well begin our mathematical discussion at this point, which we can do by noting the form which these restrictive conditions usually take in practice. If we denote by p_j the probability associated with the j'th class (for example, the area of the segment corresponding to the j'th age group in Figure 1, or the probability associated with heads or tails respectively in the coin-tossing case); by n_j the number of experimental observations falling in this class; and by m the total number of observations made, we have as one relation which is always used

$$\sum n_j = m \sum p_j,$$

or if we prefer to write it so

$$\sum (n_j - mp_j) = 0. \tag{1}$$

We may also use the average of our data to determine a constant in the distribution formula. In this case we require that

$$\sum a_j n_j = m \sum a_j p_j,$$

where a_j is the measure associated with the j'th group (for example, the *age* in the case of Figure 1). This also may be written in a form similar to (1) as follows

$$\sum a_j(n_j - mp_j) = 0. \tag{2}$$

The use of higher moments, or other statistical devices for estimating parameters, generally leads to equations of the same form as (2), except that the coefficients are different. For example, if we make the standard deviation of the distribution function agree with that of the data, we get an equation just like (2) except that a_j must be replaced by a_j^2. The factor $(n_j - mp_j)$ remains unaltered, and so does the zero on the right-hand side.

In technical mathematical language this would be expressed by saying that all such equations are *linear and homogeneous in the variables* $n_j - mp_j$.

Phrased mathematically then, our problem is to test the significance of the deviation of certain data from chance expectation when a certain number of such linear homogeneous relations are known to exist.

2. THE ANSWER

So much for the nature of the problem itself. Next we must ask what might be regarded as an acceptable answer. In other words, what is it that constitutes a *significant* disagreement between data and hypothesis? The first and most obvious fact is that the answer is somehow related to the probability of getting the data from an experiment performed under our hypothetical conditions. *Other things being equal*, the smaller this probability is, the more serious the discrepancies are.

But this probability alone is not the whole answer. To see this, let us consider a coin-tossing example, and suppose that in ten throws heads has appeared only once. This certainly cannot be said to be

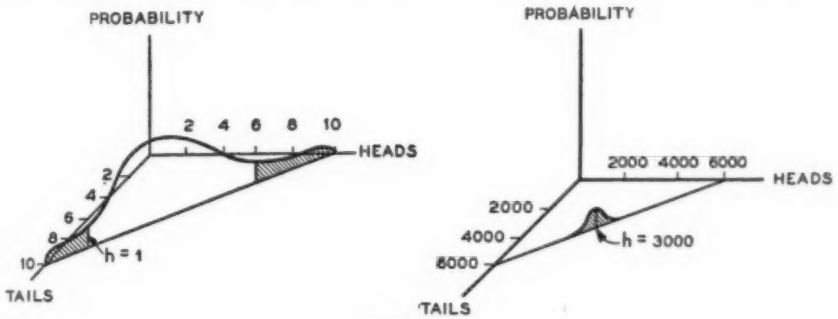

FIGURE 2

very strong support for the hypothesis that the coin is true; not nearly so strong as if heads appeared just 3,000 times in 6,000 throws. Yet the probability of each result is identical, as we see from Figure 2 in which the binomial distributions appropriate to the two cases are drawn to identical scales, and the particular probabilities appropriate to one head out of ten and 3,000 out of 6,000 are shown by the heavy ordinates.[3]

Figure 2 also gives us a clue to why the exact magnitude of the probability is not of primary importance; for in one case the heavy ordinate is at the exact peak of the curve so that the experimental result, though improbable in an absolute sense, was not at all unusual when compared with the other things that might have occurred. In

[3] It would be much more usual to plot the ordinates of Figure 2 against one variable (heads) allowing the other variable (tails) to be understood. I have chosen instead to display them as functions of two variables, between which a certain auxiliary condition exists, so that not all pairs of values are admissible. In this sense both curves are sections cut from a surface whose equation is

$$p = \frac{(h+t)!}{h!\,t!}\left(\tfrac{1}{2}\right)^h\left(\tfrac{1}{2}\right)^t$$

one section being made by the plane $h+t=10$, the other by the plane $h+t=6{,}000$.

At present this form of presentation has no peculiar merit. Later on, however, when we begin dealing with restrictive conditions such as (1) and (2) above, we shall wish to think in these terms, and it will be convenient to have the idea fixed in mind beforehand.

the other case the heavy ordinate is well down on the tail of the curve. Relatively to the other things that might have happened, it would rate as decidedly unexpected. Indeed, it is just this matter of how far down on the tail of the curve the experimental results lie, which determines how significant they are.

Pearson's purpose in inventing the χ^2-test was just exactly to estimate how far below the peak of the curve a given experiment lies. It accomplishes this end by telling what proportion of the area lies still further down, like the shaded part of Figure 2. We see at once that in the case of 6,000 trials the whole area is shaded, and hence the answer is obviously 1. The result of any run of 6,000 tosses would give results at least as unusual as the one under discussion. In the case of ten trials only, about 1/50 of the whole area is shaded, which means that with an unbiased coin only one out of fifty experiments of this sort would give results as unusual (that is, inherently as improbable) as the data before us.

Hence, to test whether an experiment significantly contradicts a hypothesis, we first compute the probability p *of obtaining the experimental data from the hypothesis by chance alone; then we imagine a great many more experiments performed under the hypothetical conditions, and find the proportion,* P, *of them which would be less probable than* p. *If this proportion* P *is very small, the experiment contradicts the hypothesis significantly; if not, the hypothesis is still tenable.*

3. MATHEMATICAL FORMULATION

We now seek to put these ideas into mathematical form. To this end, we first ask what the probability is of getting n_1 out of m events in class 1, n_2 in class 2, and so on, if the probabilities associated with the various classes are all known. The answer to this question is the familiar one[4]

$$p[n_j] = \frac{m!}{n_1!n_2! \cdots n_s!} p_1^{n_1} p_2^{n_2} \cdots p_s^{n_s}, \tag{3}$$

where s is, of course, the number of classes. This is a perfectly general result, and good for all values of $[n_j]$.

Sooner or later we must talk about the experimental data actually furnished us, and in order to distinguish it from all the other things that *might* have happened and which are covered by the n's, I shall use Greek characters $\nu_1, \nu_2, \cdots, \nu_s$. Then the probability denoted by p in the italicized statement above is

$$p = p[\nu_j] = \frac{m!}{\nu_1!\nu_2! \cdots \nu_s!} p_1^{\nu_1} p_2^{\nu_2} \cdots p_s^{\nu_s}. \tag{4}$$

[4] In writing square brackets I am using the mathematical abbreviation for $(n_1, n_2, \cdots, n_s)$. In other words, $p[n_j]$ means "the probability of getting n_1 events in the first class, n_2 in the second, etc."

Our next step must be to find what proportion of experiments conducted under the hypothetical conditions would lie at least as far down on the tail of the distribution (3) as does the actual experiment $[\nu_j]$. This aspect of the problem is complicated by comparison with that revealed in Figure 2 only because we now have s variables n_j, and hence would require an $s+1$ dimensional space in which to plot the function $p[n_j]$ against them. As we live in 3-space we cannot visualize this plot in detail, but we can infer its principal characteristics from what we already know about Figure 2.

In the first place, not all sets of values $[n_j]$ are admissible because of restrictive conditions such as (1) and (2). Indeed it was exactly the restrictive condition (1) which caused us to confine our attention to the values along a certain straight line in Figure 2 instead of covering the entire plane.

In the second place, we infer from the characteristics of the binomial that $p[n_j]$ will have its maximum values in the neighborhood of the point E whose coordinates are $mp_1, mp_2, \cdots, mp_s$. As we move away from this point in any direction $p[n_j]$ will steadily decrease in value and, if m is finite, will ultimately drop to zero. Hence we can draw a closed surface S_p in the space of s dimensions such that every admissible combination $[n_j]$ for which $p[n_j] > p$ lies *inside* this surface, and all the others lie either on it or outside. Of course, this surface will pass through the point $[\nu_j]$ which corresponds to the given data.

We could also draw a great many other surfaces for other values of the probability $p[n_j]$. These would obviously fit together like the layers of an onion, the outside layer corresponding to zero probability and the inner ones to increasingly higher values, with the maximum reached at the common center of all, the point E.

In terms of this picture, the chance that an experiment, conducted under the hypothetical conditions, would fall further down on the tail of the curve than $[\nu_j]$ is simply

$$P = \sum p[n_j], \tag{5}$$

the summation being extended over all values of n_j which lie outside the surface S_p, and *on the intersection of the surfaces defined by the equations of condition* (*1*) *and* (*2*). The latter condition is necessary because, as we explained above, the fact that $[\nu_j]$ satisfies these equations is not the result of chance: it was forced to do so. Hence the requirement that $[n_j]$ also satisfy these equations becomes a part of the conditions under which our hypothetical experiments must be performed.

4. THE APPROXIMATE FORMULA

The equation (5) is exact, but in the majority of cases in which we would wish to apply it, it would be so hard to compute as to be practi-

cally useless. To reduce it to a useable form, we must content ourselves with a reasonably valid approximation. To get this, *three separate stages of approximation* will be necessary.

The first stage is to replace all the factorials in (3) by their Stirling approximations. This gives

$$p[n_j] = \frac{1}{(\sqrt{2\pi m})^{s-1}\sqrt{p_1 p_2 \cdots p_s}} \left(\frac{mp_1}{n_1}\right)^{n_1+\frac{1}{2}} \left(\frac{mp_2}{n_2}\right)^{n_2+\frac{1}{2}} \cdots \left(\frac{mp_s}{n_s}\right)^{n_s+\frac{1}{2}}. \tag{6}$$

The next step in the process—this one does not involve an approximation—is to introduce a new set of variables of such a character as to make the surface S_p as nearly spherical as possible, and at the same time transfer its center to the origin of the coordinates. This change has two advantages: it will ultimately be found to have made all the other layers of our onion spherical also, so that $p[n_j]$ will depend only on their radius variable r, instead of upon the set of s variables $[n_j]$; in the meantime it will be a great help in selecting the quantities which we may legitimately discard as unimportant. Selecting such quantities is not easy when the important range of values for one of the variables differs much from one of the others. But since the points in any one layer of the onion are of equal importance, the important range of values will obviously be the same for all variables once the onion is made round. It turns out on trial that these objectives are attained by means of the substitution

$$n_j = mp_j + x_j\sqrt{mp_j}; \tag{7}$$

and when we make this in a typical factor of (6), we get

$$\left(\frac{mp_j}{n_j}\right)^{n_j+\frac{1}{2}} = \left(1 + \frac{x_j}{\sqrt{mp_j}}\right)^{-mp_j - x_j\sqrt{mp_j} - \frac{1}{2}}. \tag{8}$$

We are now ready for our second stage of approximation. It is similar in character to the familiar one by which an expression of the form

$$\left(1 + \frac{x}{m}\right)^m$$

is replaced by e^x when m is large. In the present case, the result turns out to be

$$\left(\frac{mp_j}{n_j}\right)^{n_j+\frac{1}{2}} \doteq e^{-x_j\sqrt{mp_j} - \frac{1}{2}x_j^2}. \tag{9}$$

When we multiply together the s factors of this sort which appear in (6), the result becomes even simpler; for we see at once from (7) that

$$\sum \sqrt{mp_j}\, x_j = \sum (n_j - mp_j);$$

and the universal condition (1) tells us at once that this is zero at all admissible points. Hence (6) becomes

$$p[n_j] \doteq \frac{1}{(\sqrt{2\pi m})^{s-1}\sqrt{p_1 p_2 \cdots p_s}}\, e^{-\frac{1}{2}(x_1^2 + x_2^2 + \ldots + x_s^2)}. \tag{10}$$

We may now note that the change of variable (7) has indeed made all the sections of our onion spherical, since $p[n_j]$ has the same value at all points of the hypersphere

$$x_1^2 + x_2^2 + \cdots + x_s^2 = r^2. \tag{11}$$

Our third approximation has to do with the summation by which P is obtained in (5), and amounts merely to replacing this discrete sum by an integral over the region in which the admissible points lie, that is, the region outside the surface S_p. But this surface, like all the other layers of our onion, is now spherical, and hence characterized by the value of its radius alone. If we denote by

$$\xi_j = (\nu_j - mp_j)/\sqrt{mp_j}$$

the particular values of the x's which correspond to our experimental data, and write (as is usually done)

$$\chi^2 = \xi_1^2 + \xi_2^2 + \cdots + \xi_s^2, \tag{12}$$

we see at once that the radius of S_p must be χ, for we have already noted that the point $[\nu_j]$ lies on this surface.

Hence the integral which we must compute takes the very simple form

$$P = K \int_{r=\chi}^{\infty} e^{-\frac{1}{2}r^2} dV. \tag{13}$$

I have here written a constant K to take care not only of the constant multiplier in (10) but also of any factor that may be introduced in changing from an element of volume in the variables $[n_j]$ to an element of volume in $[x_j]$. As the variables are not all independent, it would be a bit troublesome to compute this factor at this time, but we know it must be a constant since the equations connecting x_j and n_j are all linear.

We are now done with our approximations, but we must still express our element of volume dV in an explicit form before (13) can be computed. To do this, we must examine our "admissible region" a bit more closely. We have already said that it is composed of those points in the space of the s variables $[x_i]$ which lie on the intersection of the surfaces corresponding to the equations of condition. The latter, however, are known to be linear and homogeneous in the variables $n_i - mp_i$; and from (7) we see that this means they are linear and homogeneous in x_i. Now *linear* equations represent hyperplanes, and when they are *homogeneous* the planes pass through the origin; and because of these two peculiarities the admissible region turns out to be a very simple one, as we can readily see by analogy with the more familiar three-dimensional case.

If we consider the three variables x_1, x_2, x_3 the surface S_p becomes an ordinary sphere of radius χ with its center at the origin. A single plane through the origin cuts this in a circle, also of radius χ, and if this plane represented the only equation of condition the admissible region for the x's would be the portion of this plane for which $r \geqq \chi$. Two conditions on the other hand would correspond to a pair of planes through the origin. They would both be fulfilled by the points of the line at which these planes intersect. This line also passes through the origin and hence the admissible region is that part of the line along which $r \geqq \chi$.

In other words, one condition has reduced us from three dimensions to two, two conditions from three dimensions to one; and we naturally infer (and quite correctly) that in the most general case q conditions would reduce us from a space of s dimensions to one of $s' = s - q$ dimensions. Hence, in (13) dV *is a volume element in space of* s′ *dimensions.* We also infer (and again quite correctly) that in this reduced space of s' dimensions we are to integrate over all values of r which exceed χ.

Next we seek a more explicit expression for the volume element dV. Since the integrand depends only on r, it is appropriate to take for dV the volume between two hyperspheres of radius r and $r+dr$. We are quite accustomed to doing this in two or three dimensions, and again we can get a clue to the size of this volume element from the consideration of these familiar spaces of low degree. When we integrate a function of r in 3-space we know that the element of volume is $\pi r^2 dr$ (that is, the area of a sphere of radius r multiplied by the distance dr); in 2-space it is $2\pi r\ dr$ (that is, the circumference of a circle of radius r multiplied by the differential distance dr) while on a line it is simply $2dr$. In other words, in a space of s' dimensions we would expect it to be (and again correctly so) $r^{s'-1}$ multiplied by some constant, the

exact value of which we may allow to be absorbed by that very useful arbitrary constant K. We thus obtain

$$P = K\int_{\chi}^{\infty} r^{s'-1}e^{-\frac{1}{2}r^2}dr. \tag{14}$$

Only K is now undefined, and it is easily determined. For obviously the proportion of experiments in which $x_1^2+x_2^2+\cdots+x_s^2 \geqq 0$ must be 1; hence

$$\frac{1}{K} = \int_{0}^{\infty} r^{s'-1}e^{-\frac{1}{2}r^2}dr,$$

which is known to be

$$\frac{1}{K} = 2^{\frac{1}{2}s'-1}(\tfrac{1}{2}s' - 1)!$$

I shall not bore you by explaining how one goes about reducing the integral (14) to numerical form. The processes are tedious and without either mathematical or statistical interest. It is only important to know that they lead at the end to double-entry tables with s' and χ^2 as parameters. You are all no doubt familiar with these, but some of you at least may not have realized that they represent a working approximation to the more exact formula (5), which in spite of its apparent simplicity is actually too complicated for practical use.

5. RÉSUMÉ

In concluding my talk, it may be wise to review hastily the major ideas, which have perhaps been somewhat obscured by the necessity from time to time of dwelling on details.

First: The problem which we are seeking to solve is that of determining whether certain data could reasonably be expected to be the chance result of certain hypotheses; in other words, whether the data do or do not contradict the hypotheses to a significant degree.

I must again stress the fact that it is *disagreement* between data and hypotheses which is important. That gives reason for thinking the hypotheses are wrong. *Agreement*, on the other hand does not mean that the hypotheses are right, for there are usually many other sets of hypotheses that would agree equally well. It is only when agreement is so good that we would hardly expect it to come by chance—say when P is 0.99 or greater—that it can be said to carry implications, and then only because such figures frequently result from a misuse of the theory.

For example, if so many arbitrary constants in a distribution have been determined from the data as to leave almost no independent degrees of freedom—that is, if s and q are both large, but $s'=s-q$ is small—and if we enter the tables with s instead of s', we will almost certainly get such a result. Or if we compare the entries in a table of logarithms with the function $\log x$ the χ^2-test will gives us $P=1$, since in this case there is obviously no disagreement. But equally obviously we have no right to make the test, for the binomial law (3) is the law of *repeated independent trials*, and hence we are only warranted in applying the χ^2-tables to data that are known to result from trials of this kind.

Both of the preceding examples involve misuse of the χ^2-test. We add still a third example where the test itself is correctly used, though, as it happens, in this case the tables are not needed and could not be applied. Suppose the 52 cards of a deck are dealt and their order noted. We ask whether the result deviates significantly from chance; that is, whether there is evidence of control. Now, as all orders are equally likely, the proportion of chance orders which are as improbable as the one observed is 1; and this is so no matter whether the cards fell in a very inconspicuous order or whether they fell in the rather conspicuous one in which they came from the maker.[5] Here the answer $P=1$ conveys no important information—so far as purely statistical ideas are concerned no order could possibly be significant.

When P is very large, then we may wisely reexamine our procedure to see if it is in error. But if we find that it is not, the high value of P is of no particular significance.

Second: The criterion by means of which significance is tested is the proportion of chance experiments which would disagree with the hypotheses to at least as remarkable a degree as the one under test. I

[5] It may be well to point out that in tests of a posteriori probability such as Bayes' Theorem, the logical situation at this point would be very different. We expect a stacked deck to possess conspicuous peculiarities of order, and hence we would find under the circumstances mentioned in the text that Bayes' Theorem would lead to a presumption that the deck was stacked, or at least to a stronger probability in that direction than existed before the experiment was performed. Exactly which of these situations occurred would depend on the a priori probabilities that enter the computation.

I mention Bayes' Theorem in this connection because it emphasizes the necessity for a common sense interpretation of statistical tests which ignore the frequently intangible prior probabilities. The χ^2-test of significance belongs to this class, and I rather deprecate the tendency to adopt an iron-clad rule as to the value of P which represents the border line of significance. For ordinary situations the commonly adopted $P=0.01$ is probably reasonable enough, but in work like Rhine's where a strong prejudice (I use the word here in its etymological sense) exists against the occurrence of ESP, much smaller values of P and a much more impressive array of data generally may properly be demanded.

It is only fair to add that I believe Professor Rhine also holds to this opinion. At all events, he has extended his studies far enough to obtain values of P (some of them smaller than 10^{-20}) which would appear to be conclusive enough to silence the most ardent exponent of the thesis that his results have been obtained by chance. Whether the explanation which Rhine advances is the correct one or whether a better one exists cannot, of course, be determined by significance tests alone for as we have already strongly emphasized, the function of such tests is to disprove hypotheses, never to prove them.

have already framed this idea in several different forms: let me add another which, though redundant, is nevertheless illuminating. *The Pearson criterion of significance is the proportion of chance experiments which would contradict the hypotheses at least as significantly as the data do.*

In this connection, I wish only to emphasize once more that the criterion of itself goes no further than this: in particular it says nothing about the relative merits of several hypotheses. I may illustrate this by the playing card example already referred to. We may assume that the observed order was that of a new pack and test two sets of hypotheses: first that the result is accidental, second that an attempt was made to order the cards in just this way. We have already seen that $P=1$ on the basis of the chance hypothesis. It cannot possibly be higher for the other, but this certainly does not mean that the other hypothesis is not the more tenable one; for due to psychological factors we would expect a manipulator to be attracted by this order, so that the prior probability of manipulation is high, and the Pearson test pays no attention to prior probability.

SOME DIFFICULTIES OF INTERPRETATION ENCOUNTERED IN THE APPLICATION OF THE CHI-SQUARE TEST*

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The remarks that I have to make are not derived from any considerations of the mathematics of the chi-square test.[1] I have a considerable interest in mathematical statistics, but very little competency in it. You will not hear anything about cards or black and white balls from me. I shall speak as a practitioner who has frequently applied the test to real observations, made seriously for the solution of concrete scientific problems. I have used the chi-square test to help make decisions as to the character of experimental data in situations in which I had every reason to think it was appropriate. I have used it in the same spirit in which we doctors use, say, the Wassermann test, to help make decisions in situations where we think a patient may have syphilis. In the course of these experiences I have encountered numerous situations in which the test did not adequately perform the function for which I thought I could use it, and I shall present a few examples *seriatim:*

I. I believe that an observant statistician who has had any considerable experience with applying the chi-square test repeatedly will agree with my statement that, as a matter of observation, when the numbers in the data are quite large, the P's tend to come out small. Having observed this, and on reflection, I make the following dogmatic statement, referring for illustration to the normal curve: "If the normal curve is fitted to a body of data representing any real observations whatever of quantities in the physical world, then if the number of observations is extremely large—for instance, on the order of 200,000—the chi-square P will be small beyond any usual limit of significance."

This dogmatic statement is made on the basis of an extrapolation of the observation referred to and can also be defended as a prediction from *a priori* considerations. For we may assume that it is practically certain that any series of real observations does not actually follow a normal curve *with absolute exactitude* in all respects, and no matter how

* A paper presented at the Ninety-ninth Annual Meeting of the American Statistical Association, Atlantic City, New Jersey, December 27, 1937.

[1] In this discussion I mean, by the chi-square test, the comparison of two sets of frequencies in which chi-square is the sum of the terms $(o-t)^2/t$ calculated from the observed and theoretical frequencies, not other tests using the chi-square distribution, such as the testing of the significance of the difference of an observed and theoretical variance.

small the discrepancy between the normal curve and the true curve of observations, the chi-square P will be small if the sample has a sufficiently large number of observations in it.

If this be so, then we have something here that is apt to trouble the conscience of a reflective statistician using the chi-square test. For I suppose it would be agreed by statisticians that a large sample is always better than a small sample. If, then, we know in advance the P that will result from an application of a chi-square test to a large sample, there would seem to be no use in doing it on a smaller one. But since the result of the former test is known, it is no test at all![2]

II. In Table 1 and Chart I are shown four series of observations of basal metabolism for humans, each representing a different situation. To each series a normal curve has been fitted and a chi-square test for goodness of fit made. Judging the results in the routine way, and using $P=0.05$ as the limit of significance, we would say that the first fit ($P=0.62$) is good, *i.e.*, not rejected; the second and third ($P=0.02$) would be rejected; about the fourth ($P=0.996$), there is a question as to what should be done. Fisher says: (*Statistical Methods*, 4th ed., p. 83) "Values over 0.999 have sometimes been reported which, if the hypothesis were true, would only occur once in a thousand trials. . . . In these cases the hypothesis considered is as definitely disproved as if P had been 0.001." I can only interpret this to mean, applied here, that the hypothesis that the distribution comes from a normal universe is to be rejected just as definitely as it would have been if the P had been 0.004.

Now, in these four instances, when I considered them from my own personal viewpoint, I actually made the following decisions: The first I considered a good fit, *i.e.*, I accepted the conclusion that on the evidence at hand these observations follow the normal curve. (I will not stop for the hair-splitting question as to whether I accept or merely do not reject. This question is operationally meaningless, for I had to tell my readers whether I thought the distribution was sensibly normal or not, on the evidence at hand. Of course I knew I might be wrong, just as I would if I made a positive diagnosis of syphilis by the Wassermann test.) In the second case I rejected the hypothesis that these observations follow the normal curve. In these first two cases,

[2] Lest this be interpreted as a comment upon all tests of significance, I should like to note, without attempting here to adequately amplify the point, that there is an important distinction between the physical connotation of a test for, say, the significance of a difference between means or variances and a chi-square difference. We conceive a *true* difference of means, or a *true* difference of variances, which corresponds to the *true* distributions. These can be operationally defined. The tests are, so to speak, comments upon our estimates of these *true* differences. But there is nothing that corresponds to a *true* chi-square difference between the *true* distributions. The chi-square corresponds to no definable specific character of the *true* distribution. It is not a descriptive parameter like the standard deviation.

then, I agreed with the routine conclusion. In the third case I did *not* reject the hypothesis that the observations follow the normal curve, and in the fourth, I accepted the hypothesis of normality with confidence. In the last two cases, therefore, there is a difference between the decision made on the routine test and what I actually did in practice.

TABLE 1

BASAL METABOLISM OBSERVATIONS

Deviation from mean in class units* (mid-value)	Series 1			Series 2			Series 3			Series 4		
	Obs.	Th.	$O-T$	Obs.	Th.	$O-T$	Obs.	Th.	$O-T$	Obs.	Th.	$O-T$
below −4	1						1			10	9.0	+1.0
−3.5	3	25.3	+1.7	2	21.1	−8.1	1	14.8	−1.8	12	10.7	+1.3
−2.5	23			11			11			17	18.4	−1.4
−1.5	53	45.3	+7.7	48	32.9	+15.1	47	39.8	+7.2	27	26.0	+1.0
−0.5	68	69.9	−1.9	53	49.5	+3.5	67	72.4	−5.4	31	30.9	+0.1
+0.5	71	69.9	+1.1	49	49.5	−0.5	80	72.4	+7.6	30	30.9	−0.9
+1.5	36	45.3	−9.3	26	32.9	−6.9	26	39.8	−13.8	26	26.0	0
+2.5	19	10.1	−0.1	12	15.3	−3.3	17	14.8	+6.2	17	18.4	−1.4
+3.5	5	6.2	+0.8	1	5.8	+0.2	4			10	10.7	−0.7
over +4	2			5						10	9.0	+1.0
Total	281	281		207	207		254	254		190	190	
Median†	−0.11	0		−0.17	0		0	0		−0.07	0	
S.D.†	1.49			1.57			1.27			2.39		
Skewness	+0.08±0.05			+0.32±0.06			0			+0.09±0.06		
χ^2	3.5			12.4			10.1			0.7		
D.F.	5‡			4			3			7		
P	0.62			0.02			0.02			0.996		

* For experiment 1 the class unit includes 2 calories per square meter per hour; for experiments 2, 3, and 4 it includes 1 calorie per square meter per hour.

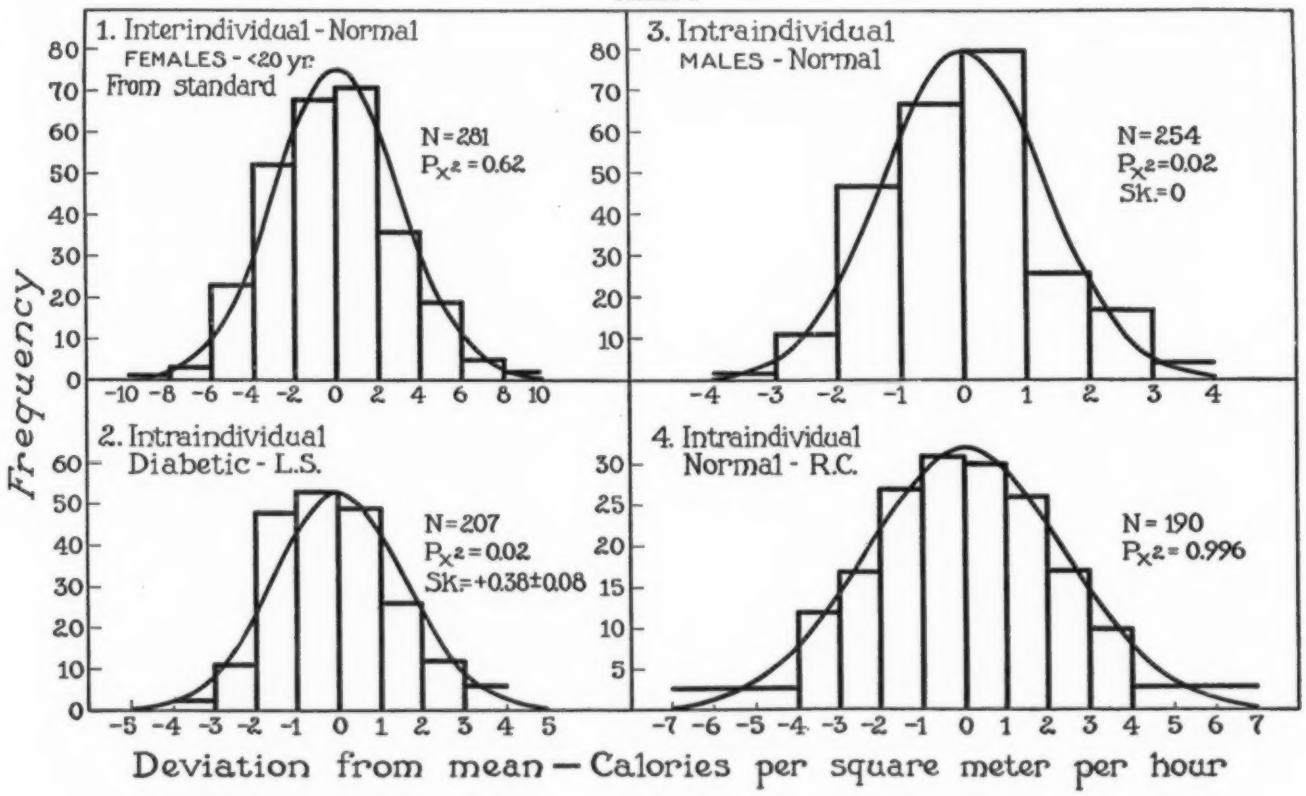
† Calculated from original data measured to 0.1 calorie, ungrouped.

‡ Deviations in this experiment are measured not from the observed but from a standard mean, and the normal curve fitted around the theoretical mean of zero, using the observed total frequency and S.D. Hence the D.F. equals classes minus 2.

If I differ with the conclusion of the test, I may inquire on what basis I made my decision and what explains my difference. For this purpose I can outline my own view as to what is the valid logical basis of the decision in any case, even the routine one. My statement of the reasoning involved will differ in certain respects, I believe, from that which would be given, say, by Fisher. Take the first two cases, in which the decisions agree. I believe the viewpoint as represented by Fisher, say, would, briefly, be something as follows:

I set up for the first case the null hypothesis that there is no difference between this distribution and a normal one. I make a test to see

CHART I


1. Interindividual - Normal
FEMALES - <20 yr.
From standard
N=281
P_{χ^2} = 0.62
3. Intraindividual
MALES - Normal
N=254
P_{χ^2}=0.02
Sk.=0
2. Intraindividual
Diabetic - L.S.
N=207
P_{χ^2} = 0.02
Sk=+0.38±0.08
4. Intraindividual
Normal - R.C.
N=190
P_{χ^2} = 0.996
Frequency
Deviation from mean — Calories per square meter per hour

how frequently such an experience as I have at hand would appear on this null hypothesis and, using the chi-square test for this purpose, I arrive at the conclusion that an experience with as large a chi-square difference would occur six times out of ten. There is, using the arbitrary limit of $P = 0.05$, no disproof of the null hypothesis. In the second case we go through the same reasoning and reach the conclusion that, on the null hypothesis that there is no difference, so large a chi-square would occur only two times in a hundred, and the null hypothesis is therefore rejected on account of the rarity of such an experience on this hypothesis.

I believe this reasoning is fallacious and that, logically, there is no ground for rejection, whatever the size of the P, if the consideration is limited to that P alone. However, since I came to the same conclusion and did, for instance, reject, in the second case, what do I think is the valid reasoning?

I would say: I have a set of observations at hand which I think may be normally distributed. (I think so because I have seen observations of a similar character that I was satisfied followed sensibly the normal curve.) I also think they may not follow the normal curve but some regular non-normal curve. (I think this may be because I have seen bodies of data like this which do not follow the normal curve but which were, for instance, skew, etc.)[3] I then make an inquiry along the following lines: If the observations come from a normal distribution, how frequently would such a chi-square as I got occur? The conclusion is, "Quite rarely—only two times in a hundred." I then make an inquiry, not stated and not calculated, but I believe absolutely necessary for the completion of a valid argument, as follows: If the distribution is non-normal, this experience, judged by a chi-square difference, would occur quite frequently. (All I have to do is imagine that the non-normal curve has the observed skew character of the distribution.) I therefore reject the normal hypothesis on the principle that I accept that one of alternative considered hypotheses on which the experienced event would be more frequent. I say the rejection of the null hypothesis is valid only on the willingness to accept an alternative (this alternative not necessarily defined precisely in all respects).

Now the line of reasoning that I have described, as contrasted with what I have described as the more usual, would explain why my decision differs from the routine one in the third and fourth cases.

[3] The importance of the consideration of a set of alternative hypotheses in statistical reasoning has been set forth very entertainingly by Fry, 1933 ("A Mathematical Theory of Rational Inference," published in *Scripta Mathematica*, II (1934), 204–221.) The argument developed here under II may be considered an application of the general viewpoint advanced by Fry to the specific question of the interpretation of the chi-square test.

With regard to the third case, after I have tried the chi-square test, I have reached the conclusion, that on the hypothesis of no difference from normality, a distribution with so large a chi-square would occur rarely. So far we are in exactly the same position as we were at this point in the second case. But now let me examine the probability that this experience would occur if the original supply were a regular non-normal one. Would this experience occur more frequently? There is no reason to say so. The distribution is perfectly symmetrical, i.e., the skewness is zero (there were exactly 50 per cent of the cases on each side of the mean), and a cursory examination of the differences from expected frequencies in the different classes shows they are not systematic, i.e., the plus deviations and minus deviations alternate in random order. Such a distribution is not to be expected frequently from any plausible non-normal curve. We therefore have *no reason at hand for rejection of the normal curve.*

My view is that *there is never any valid reason for rejection of the null hypothesis except on the willingness to embrace an alternative one.*[4] No matter how rare an experience is under a null hypothesis, this does not warrant logically, and in practice we do not allow it, to reject the null hypothesis if, for any reasons, no alternative hypothesis is credible. The fact that statisticians talk in terms of the null hypothesis and disproving it, is due to the circumstance that the numerical calculations can usually be made only with this part of the problem. The fact that the alternative hypotheses cannot be dealt with *numerically* should not lead to the fallacious conclusion that they do not form an integral part of the necessary logical structure by which the null hypothesis is rejected.

It is easy to see by the reasoning that I have given why, in my view, there is absolutely no reason for rejecting the normal fit for the fourth case. The event at hand has been proved, by the finding of a $P=0.996$, to be of a very rare kind under the normal hypothesis, surely, and on the basis of the principle that such a rare event is itself a warrant for rejection, we should reject it here, and Fisher seems to say we should.

[4] It is not necessary, for the purposes in hand, to define the alternatives at the outset. Indeed it may be economical not to do so. As the argument is advanced here, rejection of the null hypothesis depends on the acceptance of an alternative according to which the observed event would be impressively more frequent. It is understood, of course, that the alternative need not be defined completely or precisely. We will be interested, therefore, in the end, only in such other hypotheses as agree with the observations, and we may wisely wait till the analysis is made to select for consideration only such other hypotheses as are agreeable. The procedures may therefore be outlined in logical order as follows: (1) The chi-square P is evaluated; if it is not unusually low, the null hypothesis may be accepted so far as the evidence in hand is concerned. (2) If the P is unusually low, other hypotheses, agreeing with the data (i.e., which would yield a P like the one found) are to be considered. For any such a one we will then wish to know: (a) is it alternative, i.e., different from the null hypothesis in a respect pertinent to the problem in hand?; (b) does it have *a priori* plausibility? If both these conditions are fulfilled, the null hypothesis is rejected in favor of the alternative.

But in my formulation we must now ask whether the experience would be comparatively *frequent* under a non-normal distribution. Of course, the experience would be comparatively *infrequent*. If the fit seems miraculous on the hypothesis of normality, it would be even more so on a hypothesis of non-normality. Were an alternative hypothesis, to the effect that the data had been falsified to render them normal, pertinent and tenable in the circumstances, there would be ground for rejection. But even then what would be rejected is not the hypothesis but the data.

TABLE 2

DATA FROM A STUDY TO DETERMINE THE EFFECTIVENESS OF A CERTAIN VACCINE IN THE PREVENTION OF THE COMMON COLD

	Total Group			Males			Females		
	Number	Affected		Number	Affected		Number	Affected	
		No.	Per cent		No.	Per cent		No.	Per cent
Experimental	143	121	84.6	70	57	81.4	73	64	87.7
Control	157	145	92.4	80	72	90.0	77	73	94.8
Total	300	266	88.7	150	129	86.0	150	137	91.3
Difference, Control-Experimental			+7.8			+8.6			+7.1
P; χ^2 for 1 D.F.	0.02			0.07			0.06		
P; Normal distribution Difference/S.E. difference	0.02			0.07			0.06		
P; Normal distribution Mean difference/S.E. mean difference				0.02					
P; Sum of χ^2 for 2 D.F.				0.05					

III. In Table 2 are given the results from an experiment that was performed to determine the effectiveness of a certain vaccine in the prevention of the common cold. If we consider the total group, we see that the experimental group had 7.8 per cent fewer affected than did the control group. If this difference is examined by the chi-square test, we get a P of 0.02, which is significant, we will say. (Half the P of the chi-square tables is used since only positive differences are relevant here.) The same question can, of course, be answered by examining this difference with its S.E., the latter evaluated as $\sqrt{PQ(1/n_e+1/n_c)}$ where P is the rate for the total group; these two answers are identical, as we know, for the four-fold table. Now let us look at the subdivision of the data into males and females. Using the same procedures in each

four-fold table, we get for the males a $P=0.07$, not significant, and for the females $P=0.06$, not significant. We may combine the results for the two sexes by using the mean of the two differences. Examining this in the light of its S.E., we again get a $P=0.02$, the same value as before for the combined group. Looked at this way, we substantiate the previous conclusion that the significance of the difference for the experience taken as a whole is measured by a P of 0.02. Now suppose we combine the experience of the males and females by using the theorem that for independent tables we may use the sum of the chi-squares with the sum of the degrees of freedom to obtain a P. We get $P=0.05$, a value that may appear reasonable considering the P's for the males and females separately. But the P obtained this way, 0.05, is not significant, while the value previously obtained, 0.02, is. In the problem here cited there are methods available for combining the males and females to perform a chi-square test other than that of summing the chi-squares and the degrees of freedom, and I am only citing this example to demonstrate that the P obtained in the latter way may not be a good basis for judgment. I think that where the chi-square test, using the sum of the chi-squares and the degrees of freedom, fails in this example is in not being sensitive to the similar directional character of the difference. Since the chi-square function squares the differences from expectation, it destroys the value, if there be one, of knowing the sign. One can, of course, make additional different tests depending on the expectation of the distribution of signs. But my point here is that the chi-square test routinely used is not doing this and, except where you haul out an example, as I have done, you would not know it because the general direction of the results when the P is obtained by summing of the chi-squares and the degrees of freedom is reasonable. The matter becomes of practical importance when the separate tables cannot be validly combined into a single table, and this as well as another point I shall attempt to illustrate next.

IV. In Table 3 is a resume of five series of observations on the number of blood cells counted in a hemocytometer chamber. The cells in each of 400 squares of the entire hemocytometer chamber were enumerated. Since it had been domonstrated previously by "Student" (*Biometrika*, 5 (1906–1907), 351–360) that this distribution is theoretically Poisson, and this being an important matter for me to know about, I, like "Student" with his series, compared the Poisson distribution by the chi-square test for each of the five experiments. The test was made for each series in the usual way. That is, each observed mean determined a Poisson, which was used to calculate the theoretical frequency for each number of cells. The chi-square test was performed,

TABLE 3

CHI-SQUARE TEST FOR "GOODNESS OF FIT," POISSON DISTRIBUTION

Erythrocytes	Experiment I		Experiment II		Experiment III		Experiment IV		Experiment V	
	Th.	Ob.	Th.	Ob.	Th.	Ob.	Th.	Ob.	Th.	Ob.
0	↑	0	↑	0	↑	0	↑	1	↑	2
	6.92				6.77				12.45	
1	↓	5	17.31	2	↓	6	14.33	1	↓	10
2	17.81	19	↓	11	17.50	17	↓	10	27.83	21
3	35.63	33	27.64	20	35.17	38	24.05	21	49.28	52
4	53.48	49	44.86	49	53.01	52	40.56	32	65.44	63
5	64.21	59	58.25	54	63.91	65	54.72	55	69.53	77
6	64.25	78	63.03	75	64.23	69	61.53	79	61.55	62
7	55.10	57	58.46	70	55.33	46	59.32	64	46.72	46
8	41.35	38	47.44	42	41.70	40	50.03	45	31.03	41
9	27.58	29	34.22	34	27.95	28	37.51	35	18.32	11
10	16.56	19	22.22	24	16.86	22	25.31	28	9.73	8
11	9.04	9	13.12	9	9.24	10	15.53	16	↑	5
12	↑	5	↑	7	↑	5	↑	10	↑	2
13	↑	0	↑	1	↑	1	↑	3	↑	0
	8.07		13.45		8.33		17.11		8.12	
14	↓	0	↓	1	↓	1	↓	0	↓	0
15	↓	0	↓	1	↓	0	↓	0	↓	0
m	6.04		6.49		6.03		6.75		5.31	
χ^2	6.48		11.35		4.20		10.01		9.33	
D.F.	10		9		10		9		9	
P	0.77		0.25		0.94		0.35		0.41	
s	2.46	2.33	2.55	2.33	2.46	2.45	2.60	2.39	2.30	2.18
ns^2/m	357.53		332.51		396.29		338.30		357.80	
P^*	0.14		0.01		0.94		0.03		0.14	

Total $\chi^2 = 41.37$; $P = 0.71$ for 47 degrees of freedom.
Total $ns^2/m = 1782.43$; $P = 0.0006$.

* The P here is the probability of getting in random samples from a Poisson distribution so large a discrepancy between s^2 and m as that observed. The appropriate P is therefore the one corresponding to a difference from the mean chi-square as large in either direction as that observed, i.e., the P for a discrepancy in one direction is doubled. The observed chi-square is given by ns^2/m where n is the number of degrees of freedom, which for each experiment is 399, and for the total chi-square is 1995.

comparing theoretical and observed frequencies and using as degrees of freedom 2 less than the number of classes, since the theoretical distribution and the observed data agree with respect to the mean and total number. Table 3 gives the results. It is seen that in no single instance would the Poisson be rejected by the routine chi-square test, and considering the entire series together, by adding the chi-squares and the degrees of freedom we get a P of 0.71, no value for rejection. I, then, as a practitioner would say, as "Student" did say with a similar experience, that the cells followed the Poisson distribution. Having, therefore, decided to accept on the basis of the chi-square test that the Poisson distribution applies, I now go forward with my

experiment on the assumption that I can say that the standard deviation is equal to the square root of the mean, which is true for the Poisson distribution. In fact, it was in order to be able to calculate the variability from the mean that I made the test of the Poisson in the first place. It was natural for me, then, to set down for each experiment a comparison of the variance and the mean. When this was done, it was found that for each experiment the observed value of the variance was *less* than the expected value for the Poisson! A glance at the table should convince one that the S.D. is really less than $\sqrt{m}$, for in each experiment it is less, and it is very improbable that five random discrepancies would be simultaneously in the same direction by chance. But if one wishes to make statistical tests for this, they can be made in a number of ways. One can test the ratio between the mean of the differences and its standard error in the classic way, or by the t-test, and the mean difference is found to be exceedingly significant. Whatever way the test is made, in fact, the difference from the hypothesis that the variance is equal to the mean is in the order of 4 sigmas. Interestingly enough, one of the ways of testing whether the variance is equal to the mean is to use the chi-square function (not the chi-square test as it has been discussed up to now). For the Poisson distribution ns^2/m is equal to chi-square for n degrees of freedom, where n is the number of degrees of freedom used in calculating s. Table 3 shows the P value obtained by using this test for each of the experiments; and for all the experiments taken together it is 0.0006. We have now the surprising result that, considering the experiment as a whole, the chi-square test for goodness of fit of the Poisson shows no reason for rejection, whereas any test for the principal characteristic of the Poisson—namely, that the standard deviation is equal to the square root of the mean—shows indubitably that this is not true.[5]

I should attribute this discrepancy in conclusions, according as to whether they are drawn from the application of the chi-square test for goodness of fit or from a direct test for the agreement of the variance and mean, to two defects of the chi-square test considered as a test to be applied to a situation such as described here. The first is the nonspecific character of the chi-square test. The test is frequently referred to as a test for the "goodness of fit," but it is such a test only in the tautologic sense that it tests whether chi-square fits. A test can be applied only as respects a certain measurement. We recognize regularly that finding a significant difference between the mean of an

[5] "Student's" observations using yeast cells do not agree with mine in regard to the relation of the standard deviation to the mean, and, therefore, with "Student's" observations I should have drawn the same conclusions that he did. What I mean is that he did not find it necessary to supplement the chi-square test.

experiment and a hypothetical curve does not warrant rejection of the curve in other respects, say, the standard deviation. These two statistics are functions, and it is because these functions are related to certain specific physical characteristics that a significant difference in respect of them has great meaning. So, too, a significant difference tested by another function reflects a significant difference in kurtosis or skewness, etc. Every function is only a variable mathematically, and an independent investigation is required to reveal what this variable represents physically. If I say that a significant difference found by testing the probability of an experienced value of a certain variable divulges a difference only as respects the character represented by that variable, I may ask, "*What characteristic does the chi-square variable represent?*" I don't think there *is* any specific characteristic, and I believe that is one of the chief deficiencies of the chi-square test so far as its value for practice is concerned.

The second defect has to do with why, though we may be sure there is a small regular difference between the distribution of cells in the hemocytometer chamber and the Poisson, even the combined experience embracing a frequency of 2,000 hemocytometer divisions and about 13,000 erythrocytes did not divulge this as a significant difference when the various experiences were combined by summing the chi-squares and the degrees of freedom. This, I think, is another exemplification of the point I was trying to make under III. The differences were all in one direction, but the chi-square test for different independent samples combined, effected by adding the chi-squares and the degrees of freedom, was insensitive to this fact.

There is no room here for further elaboration of these views, or even for an adequate summary of the points already made. I may, however, record my impression that in practice the chi-square test is being relied on too much and too uncritically. As an exploratory tool for preliminary survey it may have some usefulness. But for any more searching analysis—as, for instance, if one wishes to base some theoretical development on the frequency function—one should seek first to ascertain functions that refer specifically to the questions at hand, and apply statistical tests that are sensitive to variations in those specific functions.

FURTHER INTERPRETATIONS OF THE CHI-SQUARE TEST

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Dr. Berkson's very interesting paper[1] raises so many questions concerning the validity and limitations of the chi-square test that it seems desirable that at least brief replies should be made to some of them. As a basis for discussion, let us first assume the following *Postulate: An hypothesis is to be rejected if the total probability of all samples as "bad" as (or worse than) the sample observed is less than some small number (say .01) previously agreed on.* This postulate is implicit in many tests of significance, various meanings being attached to the word "bad." In the chi-square test a sample is supposed to be as bad as another if it has as small a probability as the other. Moreover, both the sample and the universe are specified by their relative frequencies in a finite set of categories or "cells." This postulate is in accord with Fry's introductory remarks.[2] No attempt will be made to prove it, nor would the author be disposed to quarrel with anyone who would not accept it; for the time being at least he is only interested in the logical conclusions which flow from it.

As a first corollary of this postulate we must note that it supports Berkson in contradicting the statement of R. A. Fisher, with regard to high values of P, quoted by Berkson (p. 527); for, though the samples Fisher is thinking of have a total probability less than the critical value, these samples are none of them bad individually; they are, on the other hand, among the best obtainable; they are those that have the greatest probability. So a very large P is not to be interpreted as putting the stated hypothesis in jeopardy. It might cause one to lift the eyebrow a bit: perhaps it would pay to investigate *another* hypothesis, *viz.*, that there may have been some unnoticed forcing of the sample to make it fit the universe. But the original hypothesis is not invalidated. Such things do happen.

A second corollary of this postulate is that, since nothing is said about accepting the hypothesis when P is larger than 0.01, we cannot prove by this means that it is to be accepted in such cases. It is only not to be rejected. Acceptance is to be at most tentative, or, in Fry's words, we have only an acceptable "working" hypothesis. Berkson says that it is hair-splitting to differentiate between this kind of tentative acceptance and final acceptance, but as we shall now see it is really useful to split this particular hair.

[1] Page 526. [2] Page 513.

Berkson (p. 526) raises again the question mentioned by Fry with regard to large samples. Suppose (Fry's notation) the proportions in the universe being tested are $p_1, \cdots, p_s$, and that for a sample of $m=200$, P equals 0.05, our critical limit being 0.01. Berkson makes the categorical statement that if m is large enough (say 20,000) then it will always happen that P is less than 0.01. But, if this be so, it only means that the universe being tested is not *exactly* the universe actually being sampled, and therefore we should naturally expect to find it rejected by a very large sample. The slight difference might not be detected when $m=200$, but would always be detected when $m=20{,}000$. This is not, however, a circumstance to "trouble the conscience of an honest statistician," any more than an honest carpenter should be troubled if he uses a level which is less accurate than the astronomer's level. When P was 0.05 we were not led to accept finally the hypothesis that the universe tested was the one actually being sampled. Our acceptance was tentative, and the way was left open for a possible final rejection by a later test. We knew initially that there might be a slight difference between the two universes which could not be detected by a small sample but could be by a sufficiently large sample.

The precise relationship between the size of such a sample and the size of the corresponding detectable difference is not yet known, I think; but it is easy to show that the following equation is approximately true. Let the proportions p_j in the universe being tested all be changed by the formulae:

$$dp_j = \pm\, \epsilon, \qquad \sum dp_j = 0.$$

Then for a given sample the change in χ^2 is

$$d\chi^2 = -\,2m \sum_1^s \frac{n_j - mp_j}{mp_j}\, dp_j \quad \text{(Fry's notation)},$$

approximately, provided initially that the universe is acceptable and ϵ is very small. In a given case one can thus compute $|d\chi^2|$, but we may get a rough idea of its size now. Because the fraction is close to unity and because dp is equally often $+\epsilon$ and $-\epsilon$, it will almost always follow that $|d\chi^2| < m\epsilon$. As an example, suppose $m=200$, $s-1=14$, $\chi^2=15$, $P=0.38$. This implies an acceptable universe. We ask whether, by changing it by an amount $\epsilon=0.001$, it might be rejected by this sample. Since $d\chi^2 < (200)(.001) = 0.2$, the answer is in the negative. But if m were 20,000, $d\chi^2$ might be as large as 20, making P as small as 0.002, and the answer would be in the affirmative.

A third corollary springs from the assumption that both the universe and the sample are to be specified by their relative frequencies

in the same finite set of categories, or cells. Fry's illustration of what would happen if the number of cells were reduced to two proves conclusively that when the number is changed, the problem itself may be altered, and the answer different from what it was. This point was not made in Pearson's initial account, and there are many examples in texts and elsewhere in the literature which seem to imply that, in testing curve-fitting by this means, the chi-square test does something more than what has just been stated, but it certainly does not. One must bear this in mind in considering the judgment Berkson makes in the early part of his paper about fitting the normal curve. For such cases are not tests of the hypothesis that the universe is exactly a normal curve, but rather of the hypothesis that the universe has the various categories in the proportions indicated by the various partial areas under that curve, and over the class intervals employed. The correctness of this interpretation is evidenced by the fact that in calculating the test one makes no use of the curve itself other than to take account of the several areas mentioned. Therefore every other curve which has these same partial areas is being tested as well as the normal curve. In other words, no "curve" is being tested, only the specified set of areas. With this point in mind, let us consider Berkson's remark about the third diagram for which $P=0.02$, $N=254$. What this result means is that, if one should select a sample of 254 from a universe in which the proportions in the several class intervals were equal to the areas under that normal curve, only 2 per cent of such samples would be as bad as this one. Hence, if one's limit of acceptance is greater than 0.02, as Berkson's seems to be, one would reject this hypothesis, and one would reject it even though the skewness of the sample were zero, and even though it had certain other characteristics of a normal distribution. Of course, one might have to accept it if no other "plausible" hypothesis would lead to a greater value of P, but clearly there are plenty of such other hypotheses. In fact, every non-normal curve whose several areas are exactly equal to the histogram sketched would yield $\chi^2=0$, $P=1$. Or is there a reason why *a priori* such curves are not "plausible"?

Identical considerations provide the answer, so it seems to this author, to Berkson's Poisson cases. Experiment I, for example, does not test whether the observations are from an exact Poisson distribution, but rather whether they come from the universe indicated by the frequencies opposite the word "Theory." For this latter universe s is about what it is for the sample, if in both cases one centers the frequencies at the numbers at the top, 1, 2, $\cdots$, 12. Approximately, these two values of s are 2.40 and 2.32. But the s of the universe

sampled, 2.40, is not equal to $\sqrt{m}$; $\sqrt{m}=2.46$. The point is then that the universe being tested is not necessarily Poisson. It is Poisson only with the understanding that the frequencies 6.92 and 8.07 are spread out in special ways instead of being centered at 1 and at 12. Such a spreading-out would increase the s of the universe from 2.40 to 2.46. But in that case the frequency 5 in the sample would have to be spread out similarly, and the s of the sample would be increased. It is true, however, that cases can arise in which P can be less than 0.01 by the chi-square test, and at the same time be greater that 0.01 by the s test, and *vice versa;* another reason why acceptance is to be tentative; or, as Berkson says, both tests are "specific": χ^2 is one parameter of the sample, and s is another. It might be necessary to test the sample by both methods, and also by still other methods. If one is to use only one test, and if the number of observations is sufficiently large to warrant the use of the chi-square test, it is usually to be preferred. For this test does separate into two groups all the samples according to the probabilities of those samples; but the s test separates all the samples into two groups according to the probabilities of their respective s's. Since there exist some very improbable samples with very probable s's it is clear that these two tests do not give the same value of P.

The application to tables such as Berkson's Table I, on the effectiveness of the vaccine, is full of difficulties in interpretation. As Berkson says, a 2×2 table may be handled by the method of testing the significance of the difference between two proportions, and the results are exactly as in the chi-square test. Since the method of proportions has the advantage that, when correctly stated, it forces the attention on the precise conditions involved, let us now examine Berkson's table by that method. Also, to make the case a bit simpler, without doing violence to the real situation, let us suppose we have three tables: the first is exactly like Berkson's table for males; the second, a table for females exactly like the first; and the third, made by pooling these two.

(I) (Males)

	Affected	Not Affected	
Expt.	57	13	70
Control	72	8	80
	129	21	150

(Same with letters)

$p'N'$	$q'N'$	N'	$p'=0.814$
$p''N''$	$q''N''$	N''	$p''=0.900$
pN	qN	N	$p=0.860$

(II) (Females) Same figures as in (I)

(III) (Males and Females)

	Affected	Not Affected	
Expt.	114	26	140
Control	144	16	160
	258	42	300

(Same with letters)

	$p'M'$	$q'M'$	M'
	$p''M''$	$q''M''$	M''
	pM	qM	M

p', p'', p as in (I), $M'=2N'$, $M''=2N''$, $M=2N$.

There are three problems: (*a*) What is the probability P_a that in selecting a pair of samples of 70 and 80 respectively from a large universe in which $p=0.860$, one would get a difference in the proportions observed as great as in table I of males (or II of females), *i.e.*, as great as $|p''-p'|=0.086$? The answer is: $\chi^2_a=2.28$, $P_a=0.131$. (Berkson uses $\frac{1}{2}P$, being interested in the problem that $p''-p'\geqq 0.086$, but his explanation that the discrepancy he observed has to do with the use of the algebraic rather than the numerical difference will not hold,[3] because we shall see that the discrepancy persists when the numerical difference is used.) (*b*) What is the probability P_b that a similar result would be obtained in the pooled table III? The answer is:

$$\chi_b^2 = 2\chi_a^2, \qquad P_b = 0.033.$$

Because χ_b^2 is greater than χ_a^2, and because the number of degrees of freedom remains equal to one, it is clear that always in cases like this P for the pooled table will be less than for either of the separate tables. This is true in spite of the fact that every number in the pooled table is exactly twice the corresponding number in the first table, but it is nevertheless a result that is entirely natural. It is analogous to the fact that a throw of at least 6 heads with 8 coins is less likely than a throw of at least 3 heads with 4 coins. (*c*) What is the probability that in selecting a *pair* of samples of 150 each one would get as bad a *pair* as the two observed tables I (males) and II (females)? To answer this question we should first find the probability of the first pair and then add together the probabilities of all pairs whose probabilities were as small as that; but it may be shown that the result of this process would be exactly the same as if one used the additive chi-square method for the two tables I and II. So we let $\chi_c^2=\chi_a^2+\chi_a^2$ and use two degrees of freedom: $P_c=0.1054$. We now note (with Berkson) that $\chi_c^2=\chi_b^2$, that is, that chi-square for the additive case is the same as for the pooled table, but that, since the number of degrees of freedom is increased, the value of P is increased. We note also that this effect

[3] Unless he is referring also to the point made at the very end of this paragraph.

must always take place when a perfectly homogeneous table (p constant if the table be subdivided) is divided into any number of equal portions, and that the more portions it is divided into the greater the increase in P. This result is also to be expected, and for the following reason. In both cases we are seeking the probability of a chi-square as great as $2\chi_a^2$, but in the additive case we are including in our count some pairs of tables which would not be included in the pooled case. An example of this would be a pair in which $p'-p''$ was positive in the first component table and negative in the second, both values being large and equal numerically. The pool of such a pair would show $p'-p''=0$. In the additive case the probability of this pair would have to be included, because its additive chi-square would be large. In the pooled case its probability would not be included, for its chi-square would be zero. Again, an analogous experiment in coin tossing could be readily constructed.

In summary, then, one would conclude that Berkson's pooled table of males and females together gave him the information he desired, and that nothing was gained by splitting up the material into the two parts (except that he showed thus that the material was, in respect to differences due to sex, nearly homogeneous). The value of P which he found from additive chi-square is to be disregarded, having been made artificially large by the process of subdivision.

SOME THOUGHTS ON CURVE FITTING AND THE CHI TEST*

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1. (a) *No errors in the points. The number of points equals the number of parameters.* Consider the graph of

$$y = \phi(x; \alpha, \beta, \gamma),$$

the known function ϕ containing three parameters (or constants) α, β, and γ. Any three distinct points on the graph will return the three parameters to us, but depending on the nature of the function ϕ, it may be very difficult to carry out the solution for them. In case of

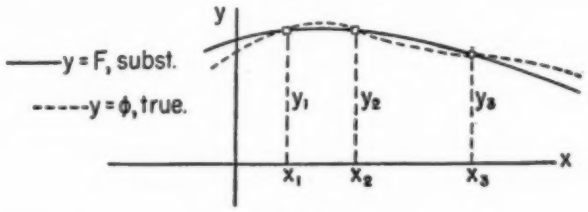

FIGURE 1
THE TRUE FUNCTION AND THE SUBSTITUTE FUNCTION; A TYPICAL SITUATION.

great difficulty it may be advisable to search for a substitute function F such that the curve

$$y = F(x; a, b, c)$$

passes through the three given points, the presumption being that the three parameters a, b, and c in F can be found without much trouble in terms of the given coordinates. Then if the remainder or discrepancy

$$R(x) = F(x; a, b, c) - \phi(x; \alpha, \beta, \gamma)$$

between the two functions is within tolerable limits, and if possibly also their slopes or higher derivatives are not too discordant over the range of the points and for a short distance on either side, we may assert that F is a satisfactory substitute or interpolation formula over this range. No general definitions of tolerable, too discordant, or satisfactory, can be laid down in advance; they will be matters of specification and personal judgment for each problem.

It is to be noted that in some circumstances, depending on the functions ϕ and F, the remainder $R(x)$ can be evaluated without great

* A paper presented at the Ninety-ninth Annual Meeting of the American Statistical Association, Atlantic City, New Jersey, December 27, 1937.

difficulty, or an upper limit found for it. In particular, if F is the polynomial $a+bx+cx^2$, the remainder is

$$R(x) = \frac{1}{3!}(x - x_1)(x - x_2)(x - x_3)\phi^{iii}(\xi)$$

where ξ is intermediate between the greatest and least of x, x_1, x_2, and x_3. But obviously the discrepancy $R(x)$ can not be evaluated unless something is known about ϕ.

1. (b) *No errors in the points. More points than parameters.* As has been remarked *any* three points on the graph of

$$y = \phi(x; \alpha, \beta, \gamma)$$

will suffice to determine the three parameters α, β, and γ, though for practical reasons it is advisable to avoid near indeterminacy by making sure that no two of the points are very close together.

But for the substitute function $F(x; a, b, c,)$ we get one set of values for a, b, and c—*i.e.* one curve—from one group of three points, and usually another set of values—*i.e.* another curve—from a different group (two of which might in fact be members of the first). Given $n>3$ points, we can find perhaps many F curves, but only one ϕ curve. Thus, when there are no errors in the points, the functions F and ϕ can be distinguished by noting which one can fit all the points simultaneously.

If there is a desire to use all n points and to obtain but one F curve, some method of compromise, such as least squares, will have to be adopted. The least squares criterion might take the form

$$\int [R(x)]^2 dx \text{ to be a minimum}$$

the integral being taken over the range of the given points. Here the square of the vertical discrepancy between F and ϕ has been minimized, but one might, with equal justification (if any), insist on minimizing the integral of the square of the horizontal discrepancy. Or, one might throw overboard any least squares criterion and use Tchebycheff's requirement that the maximum error in the vertical—*i.e.* in $R(x)$—shall be a minimum.

2. *Errors in the points.* In practice the points that we have to work with are subject to error in one or both coordinates. In contrast with the situation in the preceding section, even if the correct function $\phi(x; \alpha, \beta, \gamma)$ is known, we can not hope to get the true values of α, β, and γ from three points or from all n points. It will now be impossible to distinguish the two functions F and ϕ on the basis that the latter will fit all the points, for it will not do so except by accident if $n>3$, or in general, if n is greater than the number of parameters.

The relative weights of the observed coordinates may be known, in fact their standard errors may be known: in either of these circumstances a least squares or other adjustment is possible, by which the function $F(x; a, b, c)$ may be fitted to the observed points.

If the form of the true function ϕ were known, but not the parameters α, β, and γ, the formula

$$y = F(x; a, b, c)$$

to be fitted might be chosen identical in form with ϕ; a, b, and c being estimates of α, β, and γ obtained in fitting. Often, unfortunately, the choice of F is not dictated by considerations of ϕ, which is too frequently unknown; lacking an adequate theory for the machinery back of the data we are forced to construct an empirical formula in an attempt to describe the data.

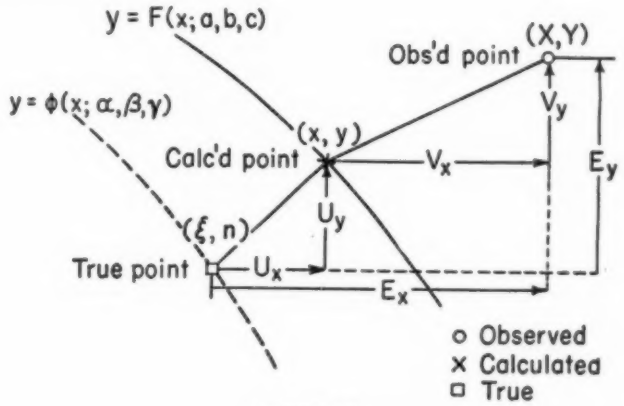

FIGURE 2

RELATIONS BETWEEN THE "TRUE," "OBSERVED," AND "CALCULATED" POINTS.

The x and y coordinates of a point are observed; these observations when plotted give the "observed point." The point that was measured is the "true point," which is unknown and lies on the unknown "true curve" $y = \phi(x; \alpha, \beta, \gamma)$. α, β, γ are the true but unknown values of the parameters. The "calculated curve" is found by adjusting the curve $y = F(x; a, b, c)$ to a series of observed points. Corresponding to each observed point there will be a "calculated point," whose coordinates are found by subtracting the "residuals" V_x and V_y from the observed coordinates X and Y. E_x and E_y denote the "errors in the observed point"; U_x and U_y denote the "errors in the calculated point." E_x, E_y, U_x, and U_y are unknown, but V_x and V_y can be calculated along with the parameters a, b, c by the method of least squares. As the figure happens to be drawn, each of the six quantities E_x, E_y, U_x, U_y, V_x, V_y is positive. Their signs are indicated by the directions of the arrows.

In the method of least squares we find the parameters a, b, and c that minimize

$$\chi^2 = \frac{1}{\sigma^2} \sum (w_x V_x^2 + w_y V_y^2)$$

for the given set of points, σ being the standard error of observations

of unit weight, w_x the weight of the x coordinate of some point, and V_x the x residual or x component of the line segment joining the observed and adjusted positions of that point. Similar definitions hold for w_y and V_y; and the summation extends over all points.

3. *The distribution of chi-square.* On the assumption that the observations on the coordinates of the points are normally distributed about the true values, and that the residuals are not large, the distribution of the minimized chi-square for repeated samples of points is[1]

$$p(\chi^2)d\chi^2 = \frac{1}{\Gamma(\frac{1}{2}k)2^{\frac{1}{2}k}}(\chi^2)^{\frac{1}{2}(k-2)}e^{-\frac{1}{2}\chi^2}d\chi^2$$

k being the number of "degrees of freedom," the number of points diminished by the number of adjustable parameters. As Fig. 2 illustrates, the residuals composing χ^2 are measured from the calculated points, which lie on the calculated curve.

The distribution of χ^2 just written holds *only if the fitted curve has the correct form* and is therefore capable of passing through the true points, in which case the average value of χ^2 arising from fitting the function F is theoretically equal to k. If any other function is fitted, the minimum χ^2 is, on the average, raised from k to some higher figure, depending on the degree of misfit.

It is instructive to hesitate for a moment on the simplest problem in curve fitting, that of n observations of equal weight on the same unknown magnitude μ, as depicted in Fig. 3. Here the y coordinate of any point is simply its ordinal number, and hence is without error. The x coordinates are supposed to be all of the same weight—unity for convenience. Here in this simple case the definition of χ^2 reduces to

$$\chi^2 = ns^2/\sigma^2$$

s being the S. D. of the n observations. $\bar{x}$, s, and χ^2 vary from one sample to another, whereupon the distribution curve for χ^2 gives immediately the curve for s. In deriving the distribution of χ^2 in 1900. Karl Pearson actually had the distribution of s in hand eight years previous to Student's semi-empirical development in 1908, both of them being antedated, however, by Helmert[2] who published the distribution of s in 1876.

[1] W. Edwards Deming, "On the chi test and curve fitting," this JOURNAL, 29 (1934), 372–382; "A new property of least squares, *Philosophical Magazine*, 19 (1935), 389–402. Occasionally the restriction that the residuals must be small can be removed; an example is the single sample of n observations (Fig. 3) for which $\chi^2 = ns^2/\sigma^2$, regardless of the size of the residuals.

[2] F. R. Helmert, *Astronomische Nachrichten*, Vol. 88, No. 2096, 1876. His development is reproduced in Czuber's *Beobachtungsfehler* (Teubner, 1891) pp. 159–163.

4. *The chi test for curve fitting.* Now if one is certain that his function F is the correct one, he would not be interested in knowing what χ^2 is, nor in making any statistical test of the function. But if he is not sure about this, he may be interested in considering how the value of χ^2 would be affected if he had been using the wrong function. He therefore makes the hypothesis that his function $F(x;\ a,\ b,\ c)$ is the correct one in form, recognizing as an alternative hypothesis the fact that the correct function may be something else. Since the alternative would, on the average, cause the value of χ^2 obtained from the

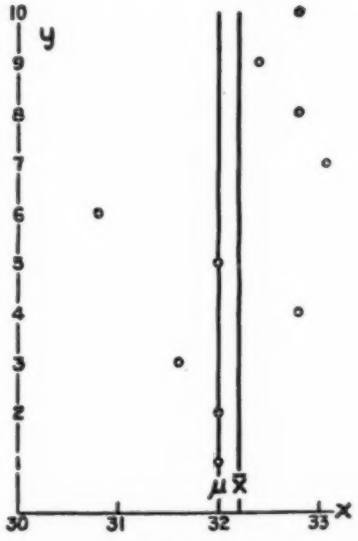

FIGURE 3
THE SIMPLEST ILLUSTRATION OF THE CHI TEST.

Ten observations of equal precision are made, supposedly all on the same unknown magnitude μ, though this is just the proposition that is to be tested. If the observations are all on the same magnitude, then the "true points" are connected by the simplest of all relations, $x = \mu$. The y coordinates are here simply the ordinal numbers of the observations, and hence are without error. $\bar{x}$ is the mean of the 10 observations; $x = \bar{x}$ is the "calculated curve," and $x = \mu$ is the "true curve." Here the chi test is merely a comparison of s with σ.

fitted function F to be higher than k, he would say that a high value of χ^2 is "evidence" that F is not the correct function. The usual tables of χ^2 and P enable one to see just how high his χ^2 really is, though in dealing with low degrees of freedom (small samples) one should bear in mind that neither the chi test nor any other test is very sensitive for detecting a false hypothesis, even in controlled experiments.

The minimized sum of weighted squares of residuals, $\sum(w_x V_x^2 + w_y V_y^2)$, is easily obtained in the solution of the normal equations, the procedure being well known, invented in fact by Gauss. Division by

σ^2 gives χ^2, as seen from the definition of χ^2 at the end of section 2. If σ is not known, the chi test can not be used on a single function, though it is still possible to compare two functions, or two sets of parameters in the same function (next section).

5. *The estimate of σ by external consistency. Gauss' criterion.* Since χ^2 is on the average equal to k if the fitted function F is the correct one, it follows that an unbiased estimate of σ^2 obtained from the fitted points is

$$\sigma^2(\text{ext}) = \sum (w_x V_x^2 + w_y V_y^2)/(n - p).$$

Here n is the number of points, p the number of adjustable parameters (hitherto taken as 3 for illustration), and $n-p$ is the number k in the distribution of χ^2 previously written.[3] This estimate of σ^2 is unbiased[4] in the sense that the average of such estimates will theoretically be equal to σ^2; being based on the consistency of the points compared with the fitted curve, it has been called by Birge[5] the "external" estimate, hence the notation just adopted.

If for brevity we now adopt the symbol S for the sum of the weighted squares of residuals, $\sum(w_x V_x^2 + w_y V_y^2)$, we may write simply

$$\sigma^2(\text{ext}) = S/k.$$

It has been remarked that when σ is not known, the chi test can not be used for a single function. The estimate $\sigma(\text{ext})$ is no help in this regard, because by its definition it leads invariably to the foreordained value k for χ^2, and hence to a value near $\frac{1}{2}$ for P; and this will be so whether the curve fits badly or well. The external estimate is accordingly useless for the chi test, since it gives a result that can be predicted with certainty before the observations are taken.

We must therefore look for an estimate of σ from other sources. Sometimes it is pretty well established from previous experience. Or, in well designed experiments, it is possible to make an estimate by what Birge calls "internal consistency"—i. e., by pooling the S. Ds. of several samples, each sample being made up from several observations on the same coordinate. Thus, if there are n_1 observations having S. D. s_1, n_2 observations having S. D. s_2, etc., we may take

$$\sigma^2(\text{int}) = \frac{n_1 s_1^2 + n_2 s_2^2 + \cdots + n_m s_m^2}{n_1 + n_2 + \cdots + n_m - m}$$

[3] Gauss himself subtracted p from n to get the corrected number of degrees of freedom for an unbiased external estimate of σ^2; the reference is to Art. 11 in his *Supplementum Theoria Combinationis* (Göttingen, 1823).

[4] The term unbiased is used in a different sense by E. J. G. Pitman, *Proceedings of the Cambridge Philosophical Society*, 33 (1937), 212–222.

[5] For some history and references see my *Least Squares* (The Graduate School, Department of Agriculture, 1938) pp. 18 and 164.

for another estimate of σ^2, also unbiased in the same sense as the external estimate is unbiased. m is here the number of samples that are combined to make the one estimate σ(int). Having both σ(ext) and σ(int) one may calculate Fisher's

$$z = \tfrac{1}{2} \ln \sigma^2(\text{ext})/\sigma^2(\text{int})$$

and use this as a criterion for testing the goodness of fit of the curve. The z test replaces the chi test when σ is not definitely known from other sources, such as from previous experience. The numbers n_1 and n_2 in Fisher's tables of z will be $n-p$ and $n_1+n_2+\cdots+n_m-m$, respectively.

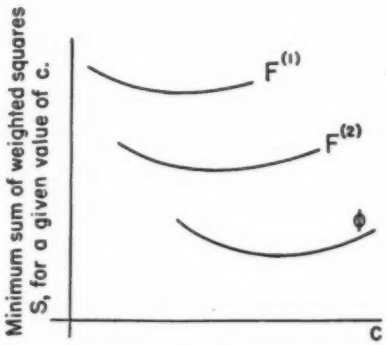

FIGURE 4

For each value of c, the sum of the weighted squares S is minimized with respect to a and b in the fitted function, and the value of S so obtained is plotted against c. The resulting curve passes through a minimum at the least squares values of a, b, and c. Here the minimized S is shown for the true function ϕ and for two substitute functions $F^{(1)}$ and $F^{(2)}$. The lowest point on the curve for ϕ is the minimum minimorum.

Owing to the fact that a high value of χ^2 is to be taken as statistical evidence that the fitted function F may not be the correct one (section 4), it follows that of two functions, or of two sets of parameters (say a_1, b_1, c_1 and a_2, b_2, c_2) for the same function, that which yields the smaller value of σ(ext) from a given set of points will have statistical preference for this set of points. This is Gauss' criterion of comparison. Since σ is a property of the observations, and not of the curve fitted to them, this criterion reduces to a comparison of two values of S (defined above).

Corresponding to a fixed value c_1 of c, there will be an a_1 and b_1 that make S as small as possible for the given set of observed points and the fitted function F. Likewise, for the value c_2 of c, there will be the values a_2 and b_2 that minimize S. Clearly, S will be a function of c, passing through a minimum at the least squares values of a, b, and c.

The shape of the curve will be nearly a parabola near the minimum. The uppermost curve in Fig. 4 shows such a plot of S for a function $F^{(1)}$. Equally, for some other function $F^{(2)}$ and the same set of points, there will be another curve for S, having similar shape, but lying more or less distant in the vertical from the first one. If the two functions are of differing types, as might be the case if they arose from different theories, and if k is large, then the vertical separation of the S curves in Fig. 4 will be relatively large compared with the change in S that is brought about by changing the parameter c through an appreciable fraction of its standard error, in either one of the two functions.

From these considerations one may conclude that Gauss' criterion is sensitive for comparing two different types of functions, but not so sensitive for comparing two values of a parameter in the same function.

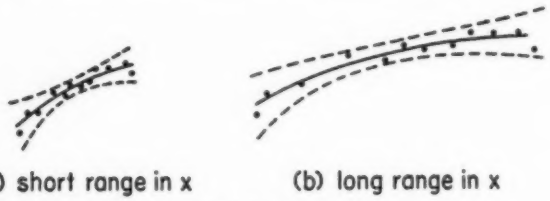

FIGURE 5

TWO RANGES IN x, BOTH GIVING THE SAME χ^2 AND P. THE DASHED CURVES SHOW THE 95 PER CENT CONFIDENCE BANDS.

Gauss' criterion for comparing two functions, or two sets of parameters in the same function, is to be used only when one is satisfied that the residuals are random. If the departures of the observed points from the fitted curve are systematic, as happens when a simple smooth curve can be drawn through the plot of deviations or residuals, then no matter what be the value of χ^2 or $\sigma(\text{ext})$, one can say at once that some other curve or further modification is required for an adequate expression of the relation existing between x and y.

6. *One further remark.* Fig. 5 shows two fitted curves, the points for one curve covering a short range in x, and for the other, a long range; the observed points, furthermore, being such that the values of χ^2 and P reckoned from them are the same for both curves. It will be agreed, I think, that there is less information contained in the short curve, in the sense that an extrapolation to the range that is successfully covered by the long curve might well be considered hazardous. Yet, as noted, both curves with their respective sets of points give the same values of χ^2 and P, and hence of $\sigma(\text{ext})$; both curves, so far as the chi test is concerned, fit their respective sets of points equally well. But the chi test takes no account of future points that may be observed

outside the range already covered, nor how those future points will lie in relation to the extrapolated curve. In the case of the curve covering the long range of points there is nothing to conjecture concerning the range already covered.

The confidence bands[6] of the curves lead to substantially the same impression; the cornucopia effect is not nearly so pronounced over the long range curve, and this fact signifies that greater confidence can be placed in the ends of curve *b* than could be placed in curve *a* if it were extrapolated to the same length as *b*.

In conclusion it is a pleasure to speak of the stimulating conversations that I have had with my friend Mr. F. F. Stephan, particularly with respect to the last section; also to mention some helpful suggestions from Professor Burton H. Camp.

[6] A confidence band for a curve is measured off as some multiple of the standard error of the calculated y, a convenient reference for which is Henry Schultz's article "The standard error of a forecast from a curve," this JOURNAL, 25(1930), 139–185. The vertical distance between the dashed curves of a 95 per cent confidence band is equal to $2t_{95}$ times the estimated standard error of the calculated y, t_{95} being obtained from Fisher's tables or otherwise, as from Nekrassoff's nomograph in *Metron*, Vol. 8 (1930), No. 3. Pages 115 and 116 of my *Least Squares* mention some other features of confidence bands.

PUNCHED CARD TECHNIQUE FOR THE CORRECTION OF BIAS IN SAMPLING

By C. W. Vickery, *Chief Statistician*
Texas Highway Planning Survey

1. INTRODUCTION

The method of stratified sampling has, during the past few years, come into extensive use in the sampling of human populations. The present paper does not undertake to extend the theory of stratified sampling, but to describe a technique for its application by means of the punched card method. This technique is also applicable to non-random samples, even if not secured by the method of stratified sampling as strictly defined. This procedure, it is thought, should be useful in studies which involve the analysis of a great quantity of data.

This technique was developed for the analysis of data from the Motor Vehicle Allocation Study, one of the studies conducted by the Texas Highway Planning Survey. The method of stratified sampling was not used in the selection of the sample. The object of this study is to obtain the distribution of certain variates associated with motor vehicles, or the owners of motor vehicles. It is desired to determine for each population group in the State the number of motor vehicles of each type whose owners are resident in that group, as well as the amount of gasoline taxes and license fees paid by such owners and other information of similar character. To obtain these data, questionnaire cards were distributed to the motor vehicle owners at the offices of the county tax collectors at the time of payment of the annual license fees. The rate of return of questionnaire cards thus varies considerably from one county to another. It is suspected, furthermore, that elements of bias may enter into the distribution within a county, —that, for example, residents of rural areas may return the questionnaire cards less well than urban residents. For this reason, it was found desirable to determine a group of controls for the preparation of "unbiased" estimates of the distribution of the desired variates in the total population of motor vehicles. The large size of the sample, over 350,000 questionnaires, required the use of the punched card method for preparing tabulations. A technique was devised for preparing, directly from the punched cards, tabulations properly adjusted to the level of the total population of motor vehicles, with the elements of bias, so far as possible, corrected. By this method, it is as easy for the tabulating machines to produce figures which represent estimates of the distributions of the required variates in the population as it is to prepare similar figures for the sample.

2. STATISTICAL METHODS

The method of stratified sampling has been developed in the past few years by Bowley, Neyman, and others. A mathematical formulation of the method is presented by J. Neyman in his paper, "On Two Different Aspects of the Representative Method."[1] The present technique represents an application of the method as formulated by Professor Neyman. This method will be described briefly in general terms. Suppose that we have determined a group of variates, which Professor Bowley has called controls, whose distributions in the sample are biased or suspected of bias, and which are correlated with the variate or variates whose distributions in the population it is desired to determine. It is assumed that the distribution of the control variates in the population is known from previous studies based on complete compilation of data or on large samples.[2] Each control variate is divided into a series of intervals, and each combination made up of one interval of each variate defines a stratum which we may think of as a cell in n-dimensional space, where n denotes the number of control variates.

Suppose that for each $i (i = 1, 2, \cdots, k)$, M_i represents the number of elements of the population and m_i the number of elements of the sample in the ith stratum. Suppose that x_{ij} represents the value of the variate x corresponding to the jth element of the ith stratum. Suppose that θ denotes the sum of the values of the variate x in the population, and θ', the best linear estimate of that sum. Neyman has shown that θ' is given by the following equation:

$$\theta' = \sum_{i=1}^{k} \sum_{j=1}^{m_i} (\lambda_{ij} x_{ij})$$

where, for each j, $\lambda_{ij} = \lambda_i = M_i/m_i$.

The application of these coefficients, $\lambda_i = M_i/m_i$, which might conveniently be called expansion factors, by means of the punched card method will be described in the next section of this paper.

3. PUNCHED CARD TECHNIQUE

For each questionnaire card, a tabulating card is punched. On this card are punched the values of the different variates which it is desired to sample, as well as certain other variates which it is expected will be used as controls. It is assumed that we have reliable control information concerning the distribution in the population of the control variates. This information would probably be compiled in the form of a two or more dimensional correlation table. Information in similar

[1] *Journal of the Royal Statistical Society*, 97 (1934), 558–625;

[2] Cf. J. Neyman: "Contribution to the Theory of Sampling Human Populations," this JOURNAL, 33(1938), 101–116.

form pertaining to the distribution of the control variates in the sample is provided by proper sorting and tabulating of the punched cards. By referring to these two collections of information, one for the population and the other for the sample, the coefficients $\lambda_i = M_i/m_i$ are calculated for the various strata.

For each i, the coefficient λ_i is punched in a vacant field of each of the tabulating cards in the ith stratum. The tabulating cards will already have been sorted in proper order in preparing the tabulation just described. The coefficients can most conveniently be punched into the cards by means of the gang punch. The coefficient for each stratum is manually punched into a master card, from which it is automatically punched into all the data cards for that stratum.

Suppose now that we wish to prepare a frequency distribution of one of the variates punched in the tabulating cards which will represent the estimated distribution of that variate in the population. To do this the cards are sorted and tabulated as they would be for preparing a similar frequency table for the sample, except that instead of having the tabulating machine count the cards in each interval, we have it tabulate the coefficients, or expansion factors, which have just been described. We can, if we so desire, have figures for the unexpanded sample in one column and figures for the expanded sample in an adjacent column.[8]

Suppose, furthermore, that we wish to produce an estimate of the average value of one of the variates in the population. We can accomplish this end most expeditiously by means of an Automatic Multiplying Punch. In producing such an average, this machine serves a function corresponding to that of the tabulating machine in producing an average based on the sample. The Automatic Multiplying Punch can be used to multiply the value of the desired variate by the expansion factor in the same card, and accumulate the sum of all these products in a counter on the machine. If now we divide this sum of products by the sum of the expansion factors for the same group of cards, we obtain a properly weighted average which represents the estimate of the average in the population. If, however, an Automatic Multiplying Punch is not available, an estimate of an average for the population can be prepared in the usual way from a frequency table, except that the frequencies are obtained by tabulating the expansion factors as described in the preceding paragraph.

4. APPLICATION

The discussion of an application of this method will be limited so far as possible to a discussion of the punched card technique that has

just been described without entering upon a discussion of the adequacy of the control variates employed. In the analysis of the portion of the Motor Vehicle Allocation Study pertaining to passenger cars, the controls employed are (1) county and (2) year model of car. If we assume that we are dealing with only one county, say Anderson County, then we have only one control, namely, year model, to think of. It has been found that the owners of the older vehicles (1) tend to return the questionnaire cards less well and (2) tend to travel fewer miles per year, with an approximately linear regression in each case.[3]

For control information, tabulations[4] showing the registration of passenger cars by year models within each county were used. Tabulations were prepared showing the distribution of the sample by year models within each county. By calculating the ratio of each registration figure to the corresponding sample figure, expansion factors were obtained for each year model within each county. For Anderson County, the following table was obtained:

Year model	Passenger car registrations	Passenger cars in sample	Expansion factor
1937	443	81	5.469
1936	721	269	2.680
1935	511	207	2.469
1934	497	166	2.994
1933	405	121	3.347
1932	170	63	2.698
1931	361	72	5.014
1930	338	89	3.798
1929	557	106	5.255
1928	261	39	6.692
Prior to 1928	269	37	7.270
Total	4,533	1,250	3.626

These factors were manually punched on master cards, and from these automatically punched into vacant columns in the data cards by means of the gang punch. By this means, the factor for Anderson County, 1937 model, is punched in all the passenger car data cards for that county and year model.

In order to obtain the distribution of the passenger cars by (human) population groups for the State as a whole, the cards are sorted by population groups, but instead of counting the cards in each group, the expansion factors are totalled for each group. To obtain the average miles traveled for a given population group (for example, places, 1,001 to 2,500 population), the cards for that group may be run through the Automatic Multiplying Punch which multiplies the miles traveled by

[3] These facts have been observed by representatives of the Bureau of Public Roads in other states. They have been confirmed in Texas.

[4] Prepared by the Reuben H. Donnelley Corporation, 350 East 22nd Street, Chicago, Ill.

the expansion factor on each card and accumulates the sum of these products in a counter on the machine. If this sum of products is divided by the sum of the factors for the same group of cards, an average miles traveled figure for that group is obtained. If an Automatic Multiplying Punch is not available, we may use a tabulating machine to prepare a frequency table showing the (expanded) number of vehicles for each coded interval of miles traveled. The frequencies are obtained by totalling the expansion factors for each interval. From this frequency table, the arithmetic mean can be calculated in the usual way. A similar procedure is employed for other variates, such as gasoline consumed and license fees paid.

THE PROBLEM OF THE STOCK PRICE INDEX NUMBER*

BY FRANCIS MCINTYRE
Cowles Commission for Research in Economics, and Colorado College

STUDENTS of index number theory cannot fail to recognize the tremendous debt they owe Irving Fisher for his services in the advancement of this field of statistical analysis. It may not be an exaggeration to say that index number science received its greatest single stimulus when Irving Fisher directed his talents toward *The Making of Index Numbers.*[1]

Recognizing this obligation, one must nevertheless voice a criticism of his purely formal approach to the selection of a "best" formula. Given that an investigator wishes to measure price movements, Fisher holds there is just one "ideal" formula, and it is expressed by

$$(1) \qquad I_i = \sqrt{\frac{\sum p_i q_0}{\sum p_0 q_0} \frac{\sum p_i q_i}{\sum p_0 q_i}}$$

where the p's and q's are prices and quantities, respectively, the subscript 0 denotes the base period, the subscript i denotes the given period for which an index is sought, and the $\sum$ denotes summation over all the items included in the index.[2]

This "ideal" formula is selected without regard for the nature of the data or the objective of the statistical analysis for which its use is recommended. It meets two formal "reversal tests" and is somewhat less complex than any of the other twelve indexes (among some two hundred considered) which also satisfy these tests.[3] This is the sole basis of its selection. In fact, the objectives of the analysis are explicitly scorned.[4]

The contrary hypothesis here urged is that it is necessary to consider the purpose for which an index is to be constructed, in the selection of the appropriate formula. Only the recognition that computers are today justifying their index number formulas on the ground that they approximate the "ideal" justifies the reaffirmation of this point which has been so ably made throughout the literature of statistics. Thus Yule says, "Professor Fisher's belief that he is able to give the correct answer to a question without knowing what the question is

* I wish to express my indebtedness to Mr. Alfred Cowles III, President of the Cowles Commission for Research in Economics, for permission to include in this paper some of the material prepared in connection with a forthcoming publication of the Commission, *Common-Stock Indexes, 1871–1937*, and for suggestions helpful in that preparation.

[1] Irving Fisher, *The Making of Index Numbers* (3rd ed.; New York: Houghton Mifflin, 1927).

[2] *Ibid.*, p. 221.

[3] *Ibid.*, pp. 219–222.

[4] *Ibid.*, p. 229.

does not appear to me well founded."[5] And again, "so far from ignoring the purpose of the index number, we must start from the purpose in order to deduce the formula required."[6] Mitchell, too, believes Fisher's position is "untenable."[7]

Bowley attacks the logic of the two reversal tests, and says they depend "simply on the author's [i.e., Fisher's] opinion that the nature of the problem or data cannot affect the question which is the best form for an index."[8]

Belcher and Flinn take a similar position in their review: "As a useful instrument an index number implies a purpose, and for different purposes the requirements are dissimilar."[9] And King says, perhaps a bit optimistically, "Once the query is made specific, the answer can also be made definite and conclusive."[10]

If this thesis be accepted, our attention may properly be turned to the consideration: What question do we *want* to answer by means of a stock price index number? We might wish to know the simple average price per share (A) of a representative list of n stocks, so that the expenditure of nA dollars on the date for which the average was calculated would purchase a single share of each issue on the list. If so, the Dow-Jones average as calculated prior to 1928 ($A_i = \sum p_i/n$) is clearly indicated.[11]

We might prefer to know the current value (nB_i) of the portfolio of an investor who purchased one share of each of the n issues on some base date, and held them continuously to the present time. The "average" relevant to this query is simply $B_i = \sum p_i/cn$, where c is the correction applied to the denominator to compensate for any changes which may have occurred in the numerator prices due to stock dividends, splits, or the issue of rights. This is the type of formula used for the Dow-Jones averages subsequent to September 10, 1928, but since c was adjusted so as to make the average the same for September 8, 1928, whether computed by the old or new formula, it appears impossible to assign a base date to which the present Dow-Jones averages may be said to refer.

[5] G. U. Yule, *Journal of the Royal Statistical Society*, 86 (1923), p. 425. The student who has not made the acquaintance of this sprightly review of Professor Fisher's book is urged to discover Yule's facile and sometimes sharp pen without delay.

[6] *Ibid.*, p. 427.

[7] Wesley C. Mitchell (U. S. Bureau of Labor Statistics Bulletin No. 284 [1921]) p. 23.

[8] A. L. Bowley, *Economic Journal*, 33 (1923), p. 93.

[9] Donald R. Belcher and Harold M. Flinn, this JOURNAL, 18 (1923), p. 930.

[10] Willford I. King, *Index Numbers Elucidated*, (New York: Longmans, 1930) pp. 30–31.

[11] Before September 10, 1928, occasional corrections were made in the Dow-Jones averages by introducing a multiplier in the numerator of A_i for the price of an individual issue which had been split up, but this was evidently somewhat haphazard. See *The Dow-Jones Averages* (8th ed.; New York: Barrons, 1938).

The question might also be asked: What was the average price per share (C_i) paid by purchasers of the n issues during period i? This calls for a weighted average, $C_i = \sum p_i q_i' / \sum q_i'$, where the q_i' are the number of shares of each of the n issues *traded* during the period in question.

However, none of these questions is the one of chief importance to the typical individual investor, who holds neither one share of each issue nor a portfolio distributed among the several issues in proportion to their volume of trading on a particular day. He wants a measure of the current value of his holdings compared with their value in some base period. For common stock investors as a group, this current value of holdings is simply $\sum p_i q_i$ where the p_i are the prevailing per share prices of all common issues, and the q_i are the numbers of shares of each issue *outstanding*. Similarly, the corresponding base period value is given by $\sum p_0 q_0$. Consequently the ratio of these values indicates the relative worth of all common stock investors' holdings, as compared with the value of such holdings on the base date, and the indicated index is

$$I_i = \sum p_i q_i / \sum p_0 q_0. \tag{2}$$

Formula (2) is the familiar value ratio of index number theory. Irving Fisher employs it to obtain index number formulas which satisfy the "factor reversal test"[12] and praises it as a useful statistic in no uncertain terms: "The value ratio . . . is not debatable . . . . It is an incontrovertible fact. There is only one value index, and it has no fringe of doubt whatever."[13]

Frisch's paper[14] also imposes the condition on any pair of price and quantity index numbers, that their product shall equal the value ratio; and J. K. Montgomery's recent work,[15] employing the same principle, makes the value ratio the basis of the entire process of formula development.

However, the suggestion that formula (2) itself be used as an index number is definitely unorthodox. Thus Fisher says, "Properly speaking, this value index . . . is not an index at all . . . ; for it is not an average of anything."[16]

Now it is precisely this feature (i.e., it is not an "average") which commends the value ratio as a stock price index number. If the index were an average of actual prices, however weighted, it would reflect price changes which are of no significance to the investor, since, e.g.,

[12] Op. cit., pp. 125 ff.

[13] Irving Fisher, "The Total Value Criterion," this JOURNAL, 22 (1927), p. 425.

[14] Ragnar Frisch, "Necessary and Sufficient Conditions Regarding the Form of an Index Number which Shall Meet Certain of Fisher's Tests," this JOURNAL, 25 (1930), pp. 397–406.

[15] *The Mathematical Problem of the Price Index* (London: P. S. King and Son, 1937).

[16] "The Total Value Criterion," loc. cit.

every stock dividend reduces such an average, although the investor has thereby suffered no loss whatever. Price alone has no such significance for common stock holders as for holders of commodities. Further, it seems reasonable to take formula (2), which measures an aggregate of investment experience, as the best measure of the average experience of the individual investor. It should be pointed out in this connection that the issues on which formula (2) is computed should constitute as nearly as possible a census of common stock investment opportunity. If it is impossible to secure nearly complete coverage of the value of outstanding issues, the sample must be selected with great care to avoid attaching too heavy a weighting to those issues with a long and active trading record.

No stock price index number formula with which the writer is acquainted escapes the necessity of some corrections for changes in the capital structure of a corporation whose issues are included in the index. But the value ratio preserves a certain consistency with regard to these corrections. As already noted, the price changes consequent upon a stock dividend or split are of no moment to the investor. The value ratio requires no adjustment for stock dividends or splits. On the other hand, the sale of additional stock by a corporation, through rights issue or otherwise, increases the value of outstanding stock, but reduces the proportionate claim of the individual investor against the corporation's earnings. The value ratio does require correction for the sale of new stock.

This correction is very simple. If the companies selling additional stock between the base period and period i realize s_i per share on their new shares issued, the current value of stock outstanding ($\sum p_i q_i$) overstates by $\sum s_i(q_i - q_0)$ the worth of the shares held in the base period. The corrected index then becomes

$$I_i = \{\sum p_i q_i - \sum s_i(q_i - q_0)\} / \sum p_0 q_0. \tag{3}$$

It may be objected that the value ratio ought not be called a stock *price* index, since it measures values (i.e., prices times quantities) and not prices alone. This is a valid objection, but against it must be set the terminological precedent established by the well-known and currently published stock price indexes (so-called) which recognize the necessity of correcting the observed prices whenever these prices fail because of changes in capital structure to reflect investor experience. Since the purpose of most (it seems fair to say all) stock price indexes is to reveal investment experience, rather than the actual course of prices, it is not unreasonable to apply the name "stock price index" to the value ratio so employed.

The only other type of correction which our index requires is that

for the inclusion of a new stock, or removal of an old issue. The value added to the numerator of (2) when the list of included issues is expanded is no profit to the individual investor; nor does he suffer any loss because it is decided to withdraw the shares of some company from the index. Consequently, the new issue added or old issue withdrawn must not be permitted to affect the index for the period in which the change (in included issues) is made.

This adjustment may be accomplished in at least two ways. A multiplier may be applied to the denominator of (2), such that the index for period i will have the same numerical value as though the list of included issues had not been changed. This is the method employed by the Standard Statistics Company.[17]

An alternative method is the construction of a "chain" index number. In the explanation of this method, it will be helpful to define $\sum^{j} p_i q_i$ as the value in period i of those stocks which are *effective* in period j. A stock is effective in period j if its price in period j is permitted to affect the index for that period. A series of links may be formed, each expressing the ratio of the value of effective stocks in a given period to the value of the same list of stocks in the preceding period. Any individual link would be

$$I_i' = \sum^{i} p_i q_i / \sum^{i} p_{i-1} q_{i-1}.$$

If these links are chained by product cumulation we have

$$I_i = I_1' \cdot I_2' \cdots I_i' = \frac{\sum^{1} p_1 q_1}{\sum^{1} p_0 q_0} \frac{\sum^{2} p_2 q_2}{\sum^{2} p_1 q_1} \cdots \frac{\sum^{i} p_i q_i}{\sum^{i} p_{i-1} q_{i-1}} \tag{4}$$

$$= \prod \frac{\sum^{i} p_i q_i}{\sum^{i} p_{i-1} q_{i-1}}.$$

As long as each link of (4) contains the same list of stocks in its numerator as in its denominator, it is clear that such list may be changed in every period without thereby introducing any arbitrary gains or losses in apparent investor experience.

Finally, the correction contained in formula (3) may be made a part of (4), thus expressing the index in its most general form. This yields

$$I_i = \prod \frac{\sum^{i} p_i q_i - \sum^{i} s_i (q_i - q_{i-1})}{\sum^{i} p_{i-1} q_{i-1}}. \tag{5}$$

[17] See any base book of the Standard Statistics Company.

An investor is "average" in the sense of formula (5) if he holds, during each period, all of the issues effective in that period, in proportion to the total value of each issue outstanding. Thus (5) becomes a more realistic measure of investment experience, the more nearly the list of effective stocks represents all outstanding issues. It therefore seems reasonable to attach more reliance to indexes which strive to make a census of the market, than to indexes which merely sample the market with 20 or 30 active issues. Thus in the period 1871–1917, the Cowles Commission Indexes are based on every industrial and public utility issue quoted on the New York Stock Exchange for two or more consecutive months, as well as 80 per cent (in value) of the railroad shares. Among regularly and currently published indexes which measure investment experience, the Standard Statistics indexes are rather in a class by themselves, with more than 400 issues whose total value is 90 per cent of that of all common stocks listed on the Big Board.

While formula (5) seems the simplest way of calculating the stock price index here advocated, identical results have been obtained by the Standard Statistics Company, using a so-called "fixed base" formula which may be expressed, in our notation, by

$$I_i = \sum^{i} p_i q_i / \prod c_i \sum^{0} p_0 q_0 \tag{6}$$

where, as before, $\prod c_i = c_1 \cdot c_2 \cdot \cdots c_i$. With this formula, the procedure is simply to calculate a multiplier c for each period in which there is a sale of additional stock by any of the included corporations, or a change in the list of included issues. This multiplier is so chosen that the sale of stock or change in list of stocks will not affect the index for the period in which they occur. Of course, if no sale or change takes place, $c=1$ for that period. Unless $c_1=c_2=\cdots=c_i=1$ it is scarcely accurate to call (6) a "fixed base" index number, although the fact that a daily or weekly index may frequently be calculated according to (6) for several months without the necessity of altering the denominator has doubtless given rise to the use of the name.

Prior to 1926, the Standard Statistics Company made its corrections for the sale of additional stock by computing for the numerator of its index a "corrected quantity weight" (say q_i'') which when multiplied by p_i yielded an adjusted value of stock outstanding ($p_i q_i''$) actually equal to $p_i q_i - s_i(q_i - q_{i-1})$. Since this corrected quantity weight had to be recomputed not only for every sale of new stock but also for every stock dividend, considerable labor was involved and the numerator of the index departed further and further from the actual value

of outstanding stock. Because of these objections, formula (6) was adopted.[18]

It may be interesting to note that the U. S. Bureau of Labor Statistics has used what is substantially formula (4) as its wholesale price index for the 29 years 1908–1936, but is just now adopting the method of adjusting numerator quantity weights whenever changes are made in an individual price series.[19]

Since it is the thesis of this article that in index numbers one man's meat is another man's poison, there is no intention of judging this change to be unwise, but the new B. L. S. method does seem to involve a rather awkward procedure.

It should be noted, in conclusion, that this paper neglects completely the brilliant recent work of Allen, Bowley, Frisch, Lerner, Staehle, and others in the field of price of living index numbers, not because the author is totally unaware of these contributions, but because such study as he has been able to make of them reveals no point at which they offer assistance in the problem of measuring investment experience in common stocks. For excellent examples of "purposeful" index numbers, however, the reader can find no better source than these discussions carried on in the various journals, chiefly perhaps the *Review of Economic Studies*, and summarized through 1935 by Frisch in the January, 1936, *Econometrica*.

SUMMARY

It is the argument of this paper that: the selection of an appropriate index number formula involves consideration of the purpose for which the index is to be employed; that the real purpose of all so-called stock price indexes is to measure not stock prices but investment experience; that the best measure of investment experience is the familiar value ratio, with simple adjustments for changes in capital structure and in the list of issues included in the index; and that the chain index formula seems the most facile in handling these corrections.

[18] See any base book, or the mimeographed description of the calculation of the Standard Statistics Indexes, prepared about 1933 and distributed by the Company.

[19] See J. M. Cutts and S. J. Dennis, "Revised Method of Calculation of the Wholesale Price Index of the United States Bureau of Labor Statistics," this JOURNAL, 32 (1937), pp. 663–674. The authors of this paper imply (p. 667) that both numerator *and* denominator quantity weights of their formula $I_k = 100\Sigma(P_kQ_h)/\Sigma(P_sQ_h)$ (where k denotes the given month, 0 the base period, and h some census date) are changed whenever a substitution (e.g., the replacement of price quotations on "Amalia" currants by those on the "Patras" variety) is made. Yet they call their formula a weighted aggregative with a fixed base. Not only does it seem a tremendous task to recompute the base value for every such substitution, but also it is not clear why such recomputation should be desired, if the value expressed by the numerator of I_k has been "adjusted so that [it is] comparable . . . with preceding periods."

PROBLEMS IN THE MEASUREMENT OF THE PHYSICAL VOLUME OF OUTPUT, BY INDUSTRIES*

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This paper is devoted to problems arising in the measurement of the physical volume of output of individual industries, not of the output of individual goods. The problems considered include: the definition of output of an industry; the measurement of output of industries incompletely reporting quantities; the place of the output of the service industries in indexes of production; and the formula and weighting scheme logical for use in combining the output of several industries. Perennial problems such as those related to the definition of an industry are not considered here. Because aggregation of quantities *per se* is not valid, by physical output we mean value of product with prices held constant.[1] An index of this value may be derived either by going directly to the physical quantities and multiplying each by the corresponding constant price, or by deflating the index of current value of product by an appropriate price index. The two methods yield identical results if the prices used in the price index relate to the same quantity units as those used in the quantity index; if appropriate formulas are used; and if the commodities covered by both methods are the same.[2]

OUTPUT OF INDIVIDUAL GOODS AND OUTPUT OF INDIVIDUAL INDUSTRIES

The physical volume of output may be considered in terms of finished products—boots and shoes, motor cars, machinery;[3] or in terms of the productive activities of industries—the output of the leather industry, of the boot and shoe industry, of the retail shoe store. It is desirable to identify the total physical volume of output based on the aggregate of finished products (including changes in inventories and the net difference between exports and imports) with the aggregate of the outputs of single industries. The distinction between the con-

* I wish to acknowledge the helpful comments of Milton Friedman and William H. Shaw.

[1] Cf. A. F. Burns, "The Measurement of the Physical Volume of Production," *Quarterly Journal of Economics* (February, 1930).

[2] The desired quantity index is $\Sigma q_1 p_0 / \Sigma q_0 p_0$. This index may be obtained either directly, or by deflating the relative of values, $\Sigma q_1 p_1 / \Sigma q_0 p_0$, by $\Sigma q_1 p_1 / \Sigma q_1 p_0$. Lack of data may force the use of a price index derived by the formula $\Sigma q_0 p_1 / \Sigma q_0 p_0$; in this case we have only an approximation to the desired price index and the derived quantity index may differ somewhat from one that would be obtained directly from the quantity data.

There is nothing specially accurate about base year prices, of course. Efforts should be made to check results by using given year prices as well.

[3] Cf. Simon Kuznets, *National Income and Capital Formation* (National Bureau of Economic Research, 1937), Part VI.

cepts then becomes significant only when the aggregates are broken down.[4] If we wish to discuss output classified by ultimate use or durability, the concept of output of finished products is the suitable one. Only a finished product is clearly classifiable in this way. Unfinished goods (steel, for example) may be used in products of diverse degrees of durability and devoted to diverse uses. If we wish to measure the output of individual industries (for example, in order to compare output with employment), the second concept is more appropriate, for it is difficult to determine the number of workers devoted to each finished product in an industry that may be turning out several commodities (some of which may be unfinished).

MEANING OF OUTPUT OF AN INDUSTRY[5]

The output of a single industry may be conceived of as either what is brought to the market by the industrial group, or what is taken off the market by the group. The grand totals of these two measures of output are identical in all years. For a single industry this equality does not hold. What is brought to the market does not equal what the industry takes back: the deflated value of the iron ore produced by the mining industry does not equal the deflated value of the butter, eggs, automobiles, and machinery that workers, stockholders, and corporations in the mining industry buy. (They are equal to one another in the base year, however, if we assume that the entire income of the group is expended on purchases of commodities and services.) We must choose between these two deflated values and make our choice explicit. Since the second offers no special conceptual difficulties, we concern ourselves with the first.

A single industry is practically never completely responsible for the output of a given commodity or group of commodities. Even farming and mining only add value to certain materials, fuels, and services which they purchase from other industries. The physical output of a single industry is therefore merely a portion of the deflated value of the goods it brings to market. It is, in other words, the deflated value it has added—the deflated net value product.[6] This deflated net value

[4] The two concepts also suggest different statistical paths by which the aggregate may be obtained; but with this we are not concerned.

[5] Cf. Simon Kuznets, "Income Originating in Nine Basic Industries," 1919–1934, *Bulletin 59* (National Bureau of Economic Research, 1936), p. 5; also A. F. Burns, *op. cit.* An interpretation similar to that given here has been presented in a recent paper (as yet unpublished) prepared by M. A. Copeland and E. M. Martin for the Conference on Income and Wealth: "The Correction of Wealth and Income Estimates for Price Changes."

[6] Capital goods are also purchased from other industries. However, it is customary, and useful, to treat capital as a factor of production. It is for this reason, also, that capital goods are treated as "finished" goods.

The net value product defined in the text is gross by the amount of capital consumption.

product is not the same as the deflated value added as defined in the Census of Manufactures. The Census subtracts only materials and fuel.[7] Payments to other industries for telephone service, typewriter ribbons, advertising, and machinery repairs are included in the Census "value added."

Just as the net value product of an industry is not measured by its gross income from sales, so is the deflated net value product not measured by the deflated gross value of product. In other words, an index of output based on the number of bags of cement produced by the cement industry is not an index of its deflated net value product. It would be so only if technical coefficients remained constant; i.e., if the quantity of rock, the amount of fuel, the number of telephone calls, etc., per bag of cement, remained constant; or if the changes in the technical coefficients happened to counterbalance one another. If economies have been effected in the use of fuel, these should be reflected in the deflated net value product.[8]

The only correct measure of the latter, then, is the deflated gross value of products minus the sum of the deflated cost of materials, fuel, etc., purchased from other industries. It is this measure which will be consistent with our identification of the total of finished goods and services with the sum of the products of single industries. If we were to ignore changes in technical coefficients the latter sum might exceed or fall short of the former.

In an industry in which increasingly successful effort is made to utilize by-products, an increase in output (as measured by deflated gross value of product) will be shown even if the quantity of materials used has remained constant. The true increase, however, will be understated by the deflated gross value; for, using the latter to measure the former implicitly assumes that the quantity of materials has increased in the same proportion as the total output.

It is not clear from the usual presentation of indexes of the physical volume of production whether they are supposed to represent the flow of goods and services gross or net with respect to capital consumption. This is another of the ambiguities surrounding most of these indexes. Probably, however, most constructors of indexes have in mind the

[7] Contract payments are shown separately and may be deducted also, as they have been in the 1935 census.

[8] "Increased economy in fuel consumption in the modernized manufacturing plants, in central light and power stations, and in railroad locomotives has been particularly marked in recent years, largely on account of improved boiler construction, the adoption of mechanical stokers, the substitution of turbines for reciprocating engines, and the use of superheating equipment. Between 1919 and 1929 the consumption of coking coal per ton of pig iron decreased 11 per cent, the amount of coal consumed per kilowatt-hour of energy generated in central power stations declined 47 per cent, and the consumption of coal per thousand gross ton-miles by Class I railroads dropped about 24 per cent." (Fifteenth Census, *Manufactures: 1929*, Volume I, p. 160.)

flow gross of capital consumption. For they think in terms of total output of apples, of dresses, and of machines, whether these are used to maintain capital or not. While both concepts are useful, we should specify the one being used. If the true net product is desired, it is necessary to subtract from the deflated value of products not only the deflated cost of materials but also the deflated depreciation and other charges for capital consumed.

The usual index of output (which is essentially an index based on deflated gross value of product) may sometimes be a fair approximation to an index of deflated net value of product. Changes in the technical coefficients relating to purchases of goods and services from other industries may in fact iron out; if not entirely, then at least in part.[9] For most industries it is probably true that economies have been effected in the use of materials and fuels. On the other hand, deflated service charges have increased more rapidly than number of units of final product, as is probably true of expenditures such as on advertising. If these changes in technical coefficients can be explicitly introduced into the computation of the physical volume of output we should not trust to the possibility that they may cancel one another. We should go beyond a first approximation based on deflated gross value of product.

THE MILLS ADEQUACY RATIO

In few industries are all products reported by type and quantity. It is therefore necessary to estimate the physical output of those products for which the quantities are not reported. For this purpose we may distinguish between two groups of industries: (1) those that report quantities for a substantial portion of their output; and (2) those that do not. F. C. Mills has introduced the "adequacy ratio" for estimating the total output of industries in the first group.[10] This is the ratio of the value, in a given year, of those products, for which quantities are reported both in the given year and in the base year, to the total value of product of the industry in the given year. Changes in this ratio are used to determine total output of those industries which report quantities for at least 60 per cent of their products (in terms of gross value).

Changes in the adequacy ratio indicate changes in the degree of coverage of the total value of output of an industry by the quantity data available. Three assumptions concerning the cause of this variation are possible.

[9] In combining the movements of different technical coefficients, to get the net change, it is necessary to weight each technical coefficient properly. The appropriate weights are related to the ratio, in the base year, of the aggregate cost of each particular item to the aggregate value of products.

[10] *Economic Tendencies in the United States* (National Bureau of Economic Research, 1932), p. 90.

(1) It may be assumed that the prices of the goods not represented by physical quantity data have fluctuated in the same degree as the prices of the goods represented. It is then a simple matter to deflate the unrepresented portion of the value of product of the industry by an index of the prices of the represented portion. (The index is obtained by dividing the value relatives of the represented goods by the corresponding quantity indexes.) This is Dr. Mills' procedure.

(2) It may be assumed that the quantity of the unrepresented goods has fluctuated in the same degree as the quantity of the represented goods. This is the assumption made when changes in the adequacy ratio are ignored.

(3) It may be assumed that neither the quantity nor the price of the group of non-represented goods has followed exactly the movements of the quantity or price of the represented goods.

In the absence of specific knowledge, the first assumption is simplest and most practicable. It is justified by the belief that prices move together within closer limits than those within which quantities move together. However, if it is possible to obtain information concerning the non-represented goods, no assumption need be made. A suitable deflator, constructed specially for the purpose, can then be applied to the values of the non-represented goods. This will only sometimes be the same as the index corresponding to the prices of the represented goods.

This discussion is relevant not only to the measurement of the gross product of an industry, but also to the measurement of the physical volume of materials, fuel, etc., consumed by the industry and thereby to the measurement of the net product of the industry.

THE OUTPUT OF INDUSTRIES WHICH REPORT NO QUANTITY DATA

For many industries, even in manufacturing, data on quantities are few or entirely lacking. These industries may be treated in several ways.

(1) An index may be computed for a sample (say, of manufacturing industries) and, implicitly or explicitly, this index may be accepted as representing all industries in the group (i.e., in manufacturing).

(2) The index of output of a given industry (say, steel) may be accepted as measuring also the output of certain related industries (e.g., machinery) and the weights of the latter imputed to the index of the former. Difficulties arise, however, because of changes in technical coefficients, changes in the proportion of, say, steel going to different steel-using industries, imports and exports of semi-manufactured goods, and changes in inventories of semi-manufactured goods. These are seldom taken into account.

(3) The sample index may be "stepped-up" to the total by means of a ratio similar to the adequacy ratio. It may be assumed that prices in the non-represented industries have fluctuated exactly as have prices in the represented industries. Or the same assumption may be made concerning fluctuations in output per worker, or value added per worker.[11]

(4) The value of product of the industries for which no quantities are available (or their cost of materials, assuming no changes in technical coefficients) may be deflated by the most suitable price indexes available. The deflators, at best, can only be crude, because (for logically best results) they should be computed as harmonic means with given year weights; this is impracticable because of the very lack of data which makes this roundabout method necessary.

It is curious that the most widely prevalent, and most inaccurate, method is the first. Less frequently used is the second. It is almost as illogical since in most cases it is applied without the corrections needed. The third has been applied in the construction of but one well-known index of output, that of F. C. Mills. The fourth has been used for one or two industries only. On a comprehensive scale, it has been applied by Simon Kuznets to the measurement of the output of finished goods.

OUTPUT OF THE SERVICE INDUSTRIES

The "service" industries offer additional difficulties which make it impossible, in practice, to avoid a cross of the concept of output of finished goods with that of industries. The contribution of some service industries to the production of physical commodities cannot be separated from the net contribution of the industries engaged primarily in turning out physical commodities—manufacturing, mining, etc. We must deal with the sum of these contributions, partly because of lack of data and partly because of the difficulty of determining what is the price or the physical unit of "service." For this reason it is impossible to measure the true deflated net product of single industries in manufacturing, etc. This means that the total output of the service industries must be broken down into two parts: (1) the output going to other business concerns, which is already included in the output of these concerns; and (2) the output going to ultimate consumers. That is, the latter quantity must be estimated. At best this can be done but roughly. But it is easier to estimate the latter quantity than to attempt to determine the "sales" by the service industries to each of the several other industrial groups.

The "service" industries include communication, professional services, services of government, finance, and real estate. Transportation

[11] The latter two are the assumptions made by F. C. Mills; see *Economic Tendencies*, pp. 39–43.

costs can be excluded from the value added in the physical-commodity industries because freight charges are considered part of the cost of materials. Real estate can also sometimes be excluded since rental payments are available by industries for certain years.

The output of the trade industries can be measured by an index such as that recently computed by Simon Kuznets.[12] This is simply the difference between the aggregate value (in 1929 prices) of goods passing out of trade and the aggregate value (in 1929 prices) of goods entering trade. Allowance is made, in this index, for changing proportions of goods handled at different gross-margin levels.

AGGREGATION OF THE OUTPUT OF SEVERAL INDUSTRIES

The concept of the output of a single industry above outlined implies a certain method of aggregation of the output of different industries. This formula is the relative of aggregates, with base year (or given year) "prices" as the constant factors. These prices are not selling prices: they represent net value added per unit of output in the base year. The prices that are usually used in combining output of individual industries are too great. Each may be too great in the same degree: the ratio of "value added" per unit, commonly used for manufacturing, to the net value product per unit in manufacturing (the correct "price") may equal the ratio of the gross value of minerals per unit of output, commonly used for mining, to the net value product per unit in mining. But this remains to be determined. We have, at least for recent years, sufficiently satisfactory estimates of the national product (the net value product gross by the amount of capital consumption) originating in specific industries so that these may be used as the basis for weights. Guesses or assumptions are unnecessary.[13]

When the output of the service industries cannot be separated from that of the physical-commodity producing industries, the weight of the indexes derived for the latter industries will be larger than the national product originating in them by the amount of the national product originating in the section of the service industries catering to them. This weight will more closely approximate "value added" as reported in the manufacturing Census, but it will still fall short of that amount.

The deflated net value products of single industries, properly computed and properly weighted as suggested above, will add up to the deflated total value of finished goods (including net changes in inventories and the difference between exports and imports).

[12] *Commodity Flow and Capital Formation* (National Bureau of Economic Research, 1938).

[13] The national income produced in each of six industrial divisions has been used as weights for combining indexes of output of these divisions by W. M. Persons and Le B. R. Foster, "A New Index of Industrial Production and Trade," *Review of Economic Statistics* (Aug. 15, 1933).

NOTES

SPECIAL MEETING OF THE AMERICAN STATISTICAL ASSOCIATION

A special meeting of the American Statistical Association was held in Ottawa, Canada, on June 28 and 29, 1938, in connection with the summer meeting of the American Association for the Advancement of Science. The meeting provided a unique opportunity for discussion of similarities and contrasts in the economic conditions and statistical work in the United States and Canada. Members of the Association from both countries participated not only in the scheduled program but in many informal discussions before and after the regular sessions. Members of the Association also attended the sessions of the section on social and economic sciences of the American Association for the Advancement of Science on the subject of "Science and the Future" and some of the sessions of other associations affiliated with the section.

Program of the Ottawa Meeting

10 A.M., Tuesday, June 28

Economic Relations of Canada and the United States

Chairman: H. D. Scully, Dominion Commissioner of Customs

The Comparability of Some of the Principal Indexes of Economic Conditions in the United States and Canada

D. C. MacGregor, University of Toronto

Influence of the United States on the Course of Business in Canada

Walter Gardner, Board of Governors of the Federal Reserve System

1:30 P.M., Tuesday, June 28

The Business Outlook with Special Reference to Government Spending

Chairman: H. A. Innis, University of Toronto

Leonard P. Ayres, Vice-President, Cleveland Trust Company

Corrington Gill, Assistant Administrator, Works Progress Administration

Courtland Elliott, A. E. Ames and Company, Investment Brokers, Toronto, Canada

10:00 A.M., Wednesday, June 29

Retail and Wholesale Trade

Chairman: Hon. Norman McL. Rogers, Minister of Labour, Canada

Some Aspects of Canadian Statistics of Merchandising and Service Establishments

H. Marshall, Chief, Internal Trade Branch, Dominion Bureau of Statistics

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Retail and Wholesale Trade

Chairman: Hon. Norman McL. Rogers, Minister of Labour, Canada

Some Aspects of Canadian Statistics of Merchandising and Service Establishments

H. Marshall, Chief, Internal Trade Branch, Dominion Bureau of Statistics

Some Comparisons of Canadian and United States Distribution Patterns
Vergil Reed, Assistant Director, U. S. Bureau of the Census
(With a discussion by both speakers of problems and plans for future distribution censuses)
The Effects of Credit and Inventories on the Flow of Goods
Willard Thorp, Director of Research, Dun and Bradstreet, Inc.

2:00 P.M., WEDNESDAY, JUNE 29

WEATHER AND CROP YIELDS
(Joint session with the Canadian Agricultural Economics Society and the Canadian Society of Technical Agriculturalists)
Chairman: T. W. Grindley, Chief Agricultural Statistics Branch, Dominion Bureau of Statistics
Crop-Weather Research in the United States
C. F. Sarle, Bureau of Agricultural Economics
Fall Precipitation and Wheat Yields in Western Kansas
George Montgomery, Kansas State College
Relations Between Weather Factors and Wheat Yields in Western Canada
C. F. Wilson, Dominion Bureau of Statistics
Discussion: J. W. Hopkins, National Research Council, Canada

The Ottawa meeting was the second in a series of special meetings of the Association which are held each summer in cities to which the annual meetings are seldom taken. The first meeting in this series was held in Denver in June 1937.

FREDERICK F. STEPHAN, *Secretary*

THE WISCONSIN COMMITTEE ON STATISTICS

With the growth of statistical work in the various departments of the Wisconsin State Government, the Executive Council was impressed with the problems arising out of possible overlapping of the various activities and the lack of coordination in such efforts and in the reporting forms of the different departments. In February, 1938, the Council appointed a special committee, consisting of the statisticians of the various departments of the state government with E. W. Morehouse of the Public Service Commission as chairman, for the purpose of studying the problems of coodinating within the state government the various efforts in this growing field.[1]

In its early meetings the committee discussed various approaches to the problems with which it was to deal, and early in March a report to the State Executive Council stressed the following points:

1. *Organization of Statistical Divisions.* It was the view of the committee that the gathering and assimilating of the statistical information by the various state agencies will be most effectively performed by maintaining the statistical divisions within each of the several state departments as autonomous units, directly responsible to the department heads.

2. *Coordinaton of Statistics.* Because of the growing use of statistical data by officials and by the general public, there is an increasing need for coordination in the field. To serve this need, an exchange of information between the statistical workers of the various departments, especially on new undertakings, will tend to eliminate duplication, perhaps eliminate some overlapping, and also make studies more comparable or facilitate joint enterprises.

3. *Reporting Forms.* Ways and means need to be found of simplifying and unifying reporting forms so as to reduce the burden of reports required or requested from individuals and organizations furnishing data to the various state and federal agencies. Duplication and overlapping of material required in reports of different governmental units should be reduced to the lowest point possible.

4. *Improvement in State Statistics.* This would include the study of problems relating to such matters as personnel, mechanical tabulation, preparation of publications and presentation, the review of work already done, and an advisory appraisal of proposed new projects.

5. *Index of State Statistical Information.* A need exists for an index of statistical information covering data gathered by the state or pertinent to the state which could be available to all departments and the public. It was proposed that this index include a description of definitions, methods, and coverage of existing series. Such an index might help to avoid duplication

[1] The members of the committee are: E. W. Morehouse, Chairman; Sidney Lerner, Secretary; C. A. Bontly, Wisconsin Conservation Commission; Walter H. Ebling, Department of Agriculture and Markets; O. A. Fried, Industrial Commission; James Hanks, Department of Commerce; Frank Hanna, Tax Commission; Arthur Jacobs, Public Welfare; Harry Jerome, University of Wisconsin; Francis E. Kester, Board of Health; L. B. Krueger, Tax Commission; Hazel K. Merriman, Banking Commission; B. O. Odegard, Board of Control; and B. J. Sickler, Public Service Commission.

and overlapping of work and be the basis of decisions regarding new work, particularly in closing gaps in existing statistical data. In addition, it would furnish a source of statistical information for the public so as to relieve the departments of the growing burden of answering current inquiries for statistical data. The committee recommended that the State Legislative Reference Library designate a research associate to devote time to this work.

Obviously, the various state statistical problems, which the committee recognized early as needing study, could not be adequately gone over in the short time during which this body has been functioning. While progress has been made toward some of the objectives, the committee itself will act mostly in an advisory capacity and will make recommendations to the Executive Council with which will rest the final decision in matters where several departments are concerned. Specific problems have been assigned to smaller committees, some of which have made reports that became the basis of recommendations to the Executive Council. Among the items already reviewed by sub-committees have been such matters as the proposed index of statistical information, mechanical tabulation, reporting forms, publication problems, and personnel. Preliminary reports have been made to the Executive Council on mechanical tabulation, reporting forms, and publication problems.

Sidney Lerner has been designated as a full-time secretary of the committee and he will also head up such research projects as the committee finds necessary to carry on. The compilation of a state statistical index is in progress and will be developed as a unit of the State Legislative Reference Library so that it may serve the various state departments and the public as well. While this work is only in its beginning, state departments have already been asked to supply lists and descriptions of their statistical series so that a start can be made in centralizing these data.

A sub-committee is working on the problem of machine tabulation and tabulating equipment. A survey has been made of the equipment available in the different departments, and a procedure has been suggested for interdepartmental use of existing state facilities. The matter of the possible establishment of a central machine tabulating unit has also been discussed.

The matter of unifying and simplifying reporting forms was undoubtedly one of the problems which was basic in the establishing of the state statistical committee. Several committee members have worked on this problem, which has been found to be extremely complex. One report has been made by this sub-committee which will result in reorganizing the work of one state department. A further project now contemplates the consolidation of reporting forms and auditing activities of three departments.

A sub-committee reviewed the problems in statistical publications and some specific recommendations looking toward reduction in cost and more effective publication of important statistical series have been made to the Executive Council. A beginning has been made in studying the problem of personnel for statistical work in the various state departments, and while some data have been collected no report has yet been made on this subject.

An important phase in the early work of this committee has been the opportunity for the members to become acquainted with the statistical problems of the various departments. Those in charge of departmental statistical work have given brief formal reviews of their fields and submitted copies of publications, schedules, etc., concerning the work in their respective departments. The broader understanding which is possible as a result of having a fuller knowledge of the statistical functions of the different departments is very helpful in facilitating the work of the committee as well as that of the members themselves. Only by knowing what is going on in the different units can the group function properly as a clearing house for problems and ideas in the state statistical set-up. Clearly the statistical data of the state will be improved when the statisticians who compile them see their significance more nearly from the viewpoint of the state government as a whole than in the light of one department's needs.

WALTER H. EBLING

Wisconsin Department of Agriculture and Markets
and U.S. Bureau of Agricultural Economics

CONFERENCE ON EMPLOYMENT AND PAYROLL STATISTICS

Sixteen state offices which cooperate with the U. S. Bureau of Labor Statistics in the collection and tabulation of employment and payroll statistics were represented at a three-day conference sponsored by the Bureau, June 20–22, 1938.[1] The meetings, held in the Department of Labor Building in Washington, were devoted to discussions of techniques and procedures in the field of employment statistics.

Similar conferences were held in June of 1937 and in June, 1936, the year which marked the expansion of the Bureau's cooperative arrangements with state agencies from collection only to cover also the joint editing and

[1] Those in attendance were: Arkansas—G. B. Segraves, Statistician, Bureau of Labor Statistics; California—Thomas F. Corcoran, B. L. S. representative with the Division of Statistics and Law Enforcement, Department of Industrial Relations; Illinois—Esther Espenshade, Division of Statistics and Research, Department of Labor, and R. S. Barker, B. L. S. representative; Indiana—C. F. Davis, Statistician, and C. F. Gross, Supervising Clerk, State Employment Service; Kansas—Nellie Kennedy, Office of Labor Commissioner, and Crystal Wheeler, B. L. S. representative; Maryland—Madeleine V. Dunne, Statistician, Office of Commissioner of Labor, and Mary Warren, B. L. S. representative; Massachusetts—Roswell F. Phelps, Director, Division of Statistics, Department of Labor and Industries, and Harry B. Sheftel, B. L. S. representative; Michigan—M. S. Cone, Statistician, Department of Labor and Industry; Minnesota—Erwin A. Gaumnitz, Director, Bureau of Research and Information, Division of Unemployment Compensation, Department of Labor and Industry, and Irene Greene, B. L. S. representative; New Jersey—Abe Rothman, B. L. S. representative with Bureau of Statistics and Records, Department of Labor; New York—Mildred A. Lauder, Statistician, and Elizabeth Corr, Division of Statistics and Information, Department of Labor; North Carolina—Pauline W. Horton, Chief, Division of Statistics, and Margaret Hillen, B. L. S., representative; Pennsylvania—Helen M. Conine, Department of Research and Statistics, Federal Reserve Bank of Philadelphia; LeRoy Fox, John L. Martin and Hugh S. Duffey, Bureau of Research and Information, Division of Labor and Industry, and Erma W. McKinney, B. L. S. representative; Texas—F. A. Buechel, Assistant Director, Bureau of Business Research, University of Texas, and Alton P. Vickery, B. L. S. representative; Wisconsin—Orrin A. Fried, Statistician, Industrial Commission, and G. A. Rothfuss, B. L. S. representative.

tabulating of the data. As early as 1922 a number of states which had pioneered in the collection of employment statistics entered into agreements with the U. S. Bureau of Labor Statistics whereby the federal Bureau furnished the postal frank for the mailing and collecting of the schedules by the states. Either the original schedules or transcripts of the data were then sent to Washington from the state office. Both state and federal tabulations were prepared from the same set of schedules, and employers were relieved of duplicate requests from two agencies.

Until 1936, however, the national and state figures were processed independently by the B. L. S. and each of the state offices. There had been but little effort toward uniformity in editing procedures, industrial classification, tabulating processes, and final presentation of the data. In May, 1936, arrangements were entered into whereby the B. L. S. furnished additional help to the state offices, usually in the form of clerical assistance, according to the needs of the state and volume of reports handled. In return, the states agreed to furnish to the Washington office, on specified dates, tabulations for inclusion in the national totals of the B. L. S. The contracts covering the arrangements specify the form of the tabulations, the editing procedures and industrial classification to be followed, the status of the federal employees in the state offices, etc.

The states represented in the cooperative set-up are, for the most part, those in which manufacturing activity is concentrated, as attested by the fact that they accounted for over two-thirds of manufacturing whether measured by establishments or employees. Nearly 70 per cent of the Bureau's manufacturing sample is included in reports received through the state offices. For nonmanufacturing industries the percentage of reports handled by the cooperating states varies, but in most cases it is above 50 per cent.

Since the inauguration of the joint tabulating procedures, the Bureau has been in constant touch with the state offices through correspondence and through visits by its staff members. In addition, it has been found profitable to the work to hold the periodic conferences of state statisticians, affording an opportunity for round table discussions of the problems which arise in connection with the work, bringing forth contributions based on the experience in each office, and for personal contact between the statisticians from the states and the B. L. S.

The recent June conference was opened by addresses by the Commissioner of Labor Statistics, Mr. Isador Lubin, on "The Wages and Hours Act—Its Statistical Implications;" by the Chief Statistician of the Bureau, Mr. Sidney W. Wilcox, on "The Uses of Employment and Payroll Statistics;" and by Miss Aryness Joy, Assistant to the Commissioner, on "The Relation of Current Employment Statistics to Social Security Reports."

There were some fifty items on the agenda. The discussion on topics of more general interest is summarized below.

The Sample. In the discussion of what constitutes an adequate sample it was pointed out that the B. L. S. strives for a coverage of at least 50 per

cent. Of the 89 manufacturing industries covered, six have a representation of over 90 per cent, 12 over 80 per cent, and in only 13 is the coverage less than 45 per cent. In industries characterized by many small establishments, such as clothing and retail food stores, it is not practicable to attempt to cover a high proportion of the total number of establishments, and the importance of weighting the sample to give these industries due influence was stressed. Weighting for large and small firms was recognized as sound procedure within those industries where establishments of varying size are found, to counteract the tendency for a few large plants to dominate the sample for those industries. State and Bureau representatives contributed to a discussion of how to build up mailing lists, second requests and other follow-up procedures, the value of personal contact with employers, effective form letters, bringing newly established companies into the reporting set-up, etc. The problem of establishing and maintaining an adequate sample for building construction received particular attention.

Adjustment to Census. The method used by the Bureau for adjusting indexes of employment and payrolls to conform to the biennial Census of Manufactures and other periodic censuses of nonmanufacturing industries was set forth in detail and mimeographed instructions were distributed. Several states are adjusting their indexes to the Census and a number of others expressed interest.

Industrial Classification. It was recognized that uniformity between state and B. L. S. classification of industries is desirable, both because it facilitates comparison between local and national changes and trends, and because it eliminates duplication of work in preparing the tabulations for the state and the B. L. S. It was admitted that some industries are important to a state report which would not be sufficiently significant to be shown separately in the national summaries; on the other hand, there are industries which are of importance in the national picture which would not appear in some states. In making adjustments in the classification for such cases, it should be possible to preserve the basic comparable arrangement.

The work of the Central Statistical Board's committee on a standard industrial classification was explained. Copies of the preliminary report for manufacturing industries were distributed. The Census of Manufacturers will probably shift to this standard classification in 1939, and the B. L. S., which bases its classification on that of the Census, will then adopt it also. The state officers were urged to consider the adoption of the standard classification as far as practicable.

Conversion factors for reducing reports to a weekly basis. The employment and payroll questionnaires call for reports "for the pay period (preferably one week) ending nearest the 15th of the month," but many firms use longer pay periods—two weeks, a half month, or a month, and report on that basis. It is necessary that all schedules be edited to an equivalent weekly figure and the problem was discussed of revising the conversion factors. This matter is to be the subject of a special memorandum.

Individual establishment reports to B. L. S. During the course of the con-

ference, arrangements were made with each state whereby the individual plant reports will be furnished each month to the B. L. S. by one of two principal procedures. Offices with machine tabulating equipment will send duplicate punched cards; others will loan their office record cards temporarily. It has been found necessary to maintain these records in the Washington office as well as in the states in order to meet special requests for information tabulated by regions, by size of plant, for a portion of one of the B. L. S. standard industry classifications, etc.

Weighted indexes for states and cities. The B. L. S. national industry averages are weighted and some of the states publish weighted indexes. The conference recognized the importance of setting up weighted indexes for states and cities. It was generally agreed that the state offices should undertake the work wherever possible.

The treatment of bonuses, vacations with pay, etc. The question of how to handle in the payroll figures items such as special bonuses, commissions, vacations with pay, vacations not taken but paid for, etc. had been particularly perplexing. The payment of bonuses to wage earners has been a growing practice. Including such irregularly occurring payments would result in marked fluctuations in average hourly and weekly earnings while wage rates actually remained constant; excluding them, on the other hand, failes to give a true picture of labor income.

It was agreed that commissions paid more or less regularly each month should be included in the payroll figures; for some types of employees commissions constitute the major portion of their pay. Bonuses, on the other hand, are not to be included unless they are paid regularly, as in the case of some production bonuses. When bonus payments are made annually, the information will be requested on a special questionnaire; these lump payments can then be taken into account for determining annual income, national income, etc. and yet not be allowed to distort the payroll index and average weekly earnings figure for any one month. Vacation pay, likewise, will be requested as a separate item; it can be added to the regular payroll figure if desired, but excluded from computations of average hourly earnings.

On the matter of perquisites and other noncash payments, representatives of the B. L. S. and the Social Security Board had conferred previous to the conference. Their recommendation, accepted by the state statisticians, was that although the Social Security Board requires perquisites to be included in payroll figures, the reports are not sufficiently comprehensive or reliable to warrant a change from the present B. L. S. policy of securing reports for cash payments only.

Recent special studies. (1) Employment and earnings reported by sex. In September, 1937 a semi-annual survey was instituted whereby the B. L. S. and cooperating states request a classification of employment, payroll, and man-hours data by sex in important woman-employing states and industries. The Women's Bureau edited and tabulated the reports. Results of the September 1937 and March 1938 surveys are available from the Women's Bureau of the Department of Labor.

(2) Office employment and payrolls. The B. L. S. has tabulated data on employment and earnings of office employees in manufacturing industries for January, June, and December of 1935 and June and December of 1936 and 1937. This information is no longer requested on the monthly questionnaire, but is secured, with the cooperation of the state offices, on a special schedule for June and December. (Some states continue to collect the data monthly for their own use.)

(3) Length of pay period. Plans were made for a special tabulation of the length of pay periods (one week, two weeks, half-month, month, etc.) in firms reporting employment. The new study applies to the month of August, 1938, and will bring to date similar studies made by the Bureau in 1929 and 1935.

(4) Full-time and part-time employment in retail trade. The schedules used by the B. L. S. and the majority of states do not request separate reports for full-time and part-time employees. As a consequence the reports including all types of employees show lower average earnings and average hours than is characteristic of normal full-time employment. New York is one state which has collected the data separately for part-time workers in retail trade. The B. L. S. has borrowed the New York schedules and made a trial tabulation which indicated significant differences between the hours and pay of the full-time and part-time employees. Consideration will be given to securing regular nation-wide reports on this basis for retail trade.

Loan copies of the minutes or information on other agenda topics can be secured upon request to S. W. Wilcox, Chief Statistician, Bureau of Labor Statistics, Department of Labor, Washington, D. C.

ERNESTINE WILKE

Bureau of Labor Statistics

CURRENT DOMESTIC TRADE STATISTICS

Additional expansion has recently taken place in the Current Statistical work of the Bureau of Foreign and Domestic Commerce. This expansion is primarily in the fields of wholesale and retail trade, and is a continuation of a program initiated in 1933 which was designed to measure the movement of the flow of goods from the producer to the consumer. The paucity of such data had long been realized both by those actively engaged in business and by students.

Retail Trade.—The Bureau's initial studies of domestic trade were in the chain store and mail order fields. Indexes were constructed for dollar sales of limited price variety chain stores, grocery chain stores, dollar volume of new passenger automobiles and rural retail sales of general merchandise. The series were prepared on a 1929–1931 base and are available by months since January 1929 both with and without adjustment for seasonal variations. The methods employed in the construction of the individual indexes have been described in various issues of the Survey of Current Business.[1]

[1] Description of the indexes can be found in the March, April and May 1934 and the May 1937 issues of the *Survey of Current Business*, Bureau of Foreign and Domestic Commerce, Washington, D. C.

These series, together with the Department Store Index of the Board of Governors of the Federal Reserve System, have a coverage of about one-fifth of total retail sales. Nevertheless, there exist many wide gaps in our information as well as the need of regional and local data. Consequently, a program designed to fill in these gaps was put into operation in the fall of 1935. This retail program envisaged the collection of independent store sales by states, by cities, and by kinds of business, and the collection of additional chain store data upon a regional basis with the ultimate objective of preparing and publishing a reliable monthly index of total retail sales.

The independent store series presented a major problem, owing to the magnitude of the task in securing a representative sample, both geographically and by kinds of business, from some 1,500,000 independent retail establishments. It was decided that data should be collected on a state basis and since the fall of 1935, when the first releases were issued, the program has been gradually developed until approximately 20,000 reports are being received each month from the 28 states covered. These states account for approximately 60 per cent of total independent store sales, and at least one state in each of the nine geographical regions as defined by the census is represented. The list of states by regions which are being covered at present is as follows:

New England:
Massachusetts

Middle Atlantic:
Pennsylvania

East North Central:
Iowa
Kansas
Missouri
Nebraska

South Atlantic:
Florida
Georgia
South Carolina

East South Central:
Alabama

West South Central:
Arkansas
Oklahoma
Texas

Mountain:
Arizona
Colorado
Idaho
Montana
Nevada
New Mexico
Utah
Wyoming

Pacific:
California
Oregon
Washington

Detailed releases are also issued for the cities of St. Louis, Chicago, Los Angeles and San Francisco. Each monthly release presents by kinds of business the percentage change from both the previous month and from the corresponding month a year ago. An aggregate of dollar sales reported is also published permitting an evaluation of the samples. In the state releases similar changes are shown by city-size groups and in those instances where a well diversified sample is available, total changes are recorded for individual cities.

The collection and tabulation of the independent store data further serve as additional basic material from which a monthly comparison of trade by kinds of business and by states can be determined. These data indicate the

variation in seasonal movement and will eventually serve as a base for the calculation of seasonal variations and the making of time series analyses.

Supplementary reports upon chain store activity which have been undertaken since October 1935 cover the drug and men's wear fields with preliminary work completed for women's wear and shoe chains. With these additions a coverage of over 50 per cent of the total chain store sales is represented and provides the basic data for the preparation of a monthly index of sales through this type of institution. Both the drug and men's wear series are shown on a regional basis with the drug series further providing comparative data for sales in tobacco and fountain departments. The men's wear information is shown not only by departments such as suits, hats, topcoats, but also by price ranges.

Since January 1938 monthly data on the cost value of stocks on hand, both in retail stores and warehouses, have been collected from chain retailers in the variety, grocery and general merchandise fields. The first public release of these stock data was recently presented together with a midyear summary of retail trade.

The Bureau's future program includes the extension of the current reporting service for independent stores into all states as well as the collection of data from other retail sources. It is also planned to collect current data on credit sales, accounts outstanding and collections made.

All of this basic material, together with that from various sources, is used in preparing the Bureau's annual estimates of total retail trade.

Wholesale Trade.—The monthly collection, analysis, and dissemination of current data reflecting conditions in wholesale trade was initiated by the Bureau in cooperation with the National Association of Credit Men. Arrangements were made with the various Federal Reserve Banks, which were collecting similar information, so that duplication would be avoided and all information of this nature eventually would be collected by the Bureau of Foreign and Domestic Commerce. The first release, which covered data for January 1936, was based upon reports from nearly 600 wholesalers who furnished comparative sales figures and data for the ratio of collections to accounts receivable outstanding. A like number of manufacturers contributed similar information. By June of 1938 the number of wholesalers contributing data had increased to 2,200, with sales for that month aggregating $179,000,000. The number of manufacturers contributing data had increased to over 1,100 with a sales volume of $198,000,000.

During 1937 intensive promotional work in wholesale trade for Electrical Goods, Drug, and Hardware resulted in greatly increased samples and the addition of information on inventories to that already being collected on sales and credit. Special trade reports presenting adequate and valid summaries in these fields were then initiated. These special summaries also include information on manufacturers and, in the Drug and Hardware bulletins, figures on retail trade are shown. Thus, in a single publication there is reviewed the current condition of a complete industry, from manufacturer through the wholesale and retail distributive channels to the ultimate con-

sumer. During the early part of this year a special monthly bulletin covering the food industry was added.

Toward the close of 1937 the increasing magnitude of the business recession resulted in a mounting interest in more adequate information with regard to inventories and their relation to sales. One of the focal points of interest in this respect was the situation in the various wholesale trades. As a result of this keen desire manifested by many business men to be better informed as to the movement of goods, the regular monthly bulletin covering all lines of wholesale trade was expanded to include inventory information by lines of business and by regions. These data were first published in the December 1937 report. With about 1,300 firms reporting their monthly inventories in the June survey, an important beginning has been made in supplying answers to the many questions on this subject which business men have been asking for years. Not only does the wholesaler have some indication of his own stock situation as compared with the average for his own trade and geographical region, but the manufacturer finds the data of extreme usefulness in preparing and carrying out his sales and marketing programs.

As of January 1, 1938, the Federal Reserve Banks transferred to this Bureau their activities in connection with wholesale trade reporting. In order that those who have been using data upon a Federal Reserve Regional basis can continue to secure it, the Bureau of Foreign and Domestic Commerce prepares a monthly tabulation for all but one of the 12 banks to be included in the bulletins of the Federal Reserve Banks. The future work of the Bureau in reference to wholesale trade will be confined to the improvement of the sample of reporting firms, to the analysis of data collected and the construction of indexes by kinds of business, together with the preparation of estimates of total wholesale trade.

EDWARD L. LLOYD

Bureau of Foreign and Domestic Commerce
U. S. Department of Commerce

THE ANNALS OF MATHEMATICAL STATISTICS

At the time of the organization of the Institute of Mathematical Statistics in 1935, the *Annals of Mathematical Statistics* was adopted as the official journal of the Institute. However, the financial reponsibility for the *Annals* was not undertaken by the Institute until this year. Final arrangements were made last April for the Institute to assume complete responsibility for the publication of the *Annals*. With the June issue of the current volume, the Institute has arranged for the *Annals* to be edited by Professors S. S. Wilks of Princeton University, A. T. Craig of the State University of Iowa, and J. Neyman of the University of California, with the cooperation of the following advisory editors: Professor H. C. Carver, University of Michigan; Professor H. Cramér, University of Stockholm, Sweden; Dr. W. E. Deming, U. S. Department of Agriculture; Professor G. Darmois,

University of Paris, France; Professor R. A. Fisher, University College, London; Dr. T. C. Fry, Bell Telephone Laboratories; Professor Harold Hotelling, Columbia University; Professor R. de Mises, University of Istanbul, Turkey; Professor E. S. Pearson, University College, London; Professor H. L. Rietz, State University of Iowa; and Dr. W. A. Shewhart, Bell Telephone Laboratories.

The *Annals* will continue to be devoted largely to original research papers dealing with topics in the mathematical theory of statistics, together with such examples as may be useful in illustrating or experimentally verifying the theory. However, in view of the purpose of the Institute of Mathematical Statistics which, interpreted broadly, is to stimulate research in the mathematical theory of statistics and to promote cooperation between the field of pure research and fields of application, plans are being made to extend the scope of the *Annals* to include expository articles from time to time on various fundamental notions, principles, and techniques in statistics. Recognizing that many theoretical statistical problems have their origin in various fields of pure and applied science and technology, papers and shorter notes dealing with theoretical aspects of statistical problems arising in such fields will be welcomed by the editors.

The editorial committee has been selected with the foregoing editorial policy in view. Furthermore, realizing the prime importance of European contributions to statistical theory, the Institute has decided to actively encourage European contributions to the *Annals* by inviting several eminent European mathematical statisticians to serve on the editorial committee.

The *Annals* will continue to be published quarterly: in March, June, September, and December. The subscription rate will remain at four dollars per year.

The *Annals of Mathematical Statistics* was established by Professor H. C. Carver, of the University of Michigan, in 1930 with the sponsorship of the American Statistical Association. During its first year it was underwritten by Mr. J. W. Edwards of Ann Arbor, Michigan, and during 1931, 1932, and 1933 it was underwritten by the American Statistical Association as a regular publication of the Association. From 1933 until this year the financial responsibility of the *Annals* was borne by Professor Carver himself. His success in establishing and continuing the *Annals* in difficult times is a remarkable accomplishment which is extremely fortunate for the Institute. It is hoped that Professor Carver's work will find its fruition in the growth of the *Annals* under the new arrangement.

JOURNAL OF THE POLISH STATISTICAL SOCIETY

The Polish Statistical Society has just published the first number of the *Statistical Review, Journal of the Polish Statistical Society*, devoted to the theory and practice of statistics. The editorial board includes Zygmunt Limanowski, Chairman; Stefan Szulc, Deputy-Chairman; Jan Wiśniewski, Secretary; Antoni Łomnicki, and Jan Piekałkiewicz. While most of the

papers are published in Polish, it is the policy of the *Statistical Review* to include papers in English, French, German, or Italian. To each paper an English summary is appended. The first issue includes the following articles, all in Polish: M. Przypkowski, The Census of Agriculture; E. Szturm de Sztrem, The Problem of Classification in Statistics; J. Czekanowski, A Contribution to the Synthesis of Cartograms; J. Wiśniewski, Remarks on the Definition of an Average; S. Szulc, The Influence of Age at Marriage on Fertility and Nativity; B. Biegeleisen, The Application of Statistical Methods in Psychology; E. Vielrose, Mortality of Eminent Persons during the Period 1000–1799. Reviews, proceedings of the Society and its sections and branches, and other notes are also included. The *Statistical Review* is published quarterly. The subscription price is zł 15 per annum.

A MANUAL OF DESIGN AND CONSTRUCTION OF TIME-SERIES CHARTS

The Sectional Committee on Standards for Graphic Presentation, sponsored by the American Society of Mechanical Engineers under the procedure of the American Standards Association, has prepared a proposed American standard for time-series charts. This manual is a rather comprehensive revision of the earlier draft issued in April 1936 under the title, "Code of Preferred Practice for Graphic Presentation—Time-Series Charts." Many of the principles of graphic presentation which are formulated in the manual for time-series charts are applicable to other types of charts. A. H. Richardson of the Bell Telephone System is Chairman of the Subcommittee which prepared the manual and is the representative of the American Statistical Association on the Sectional Committee. The manual is being published by the American Society of Mechanical Engineers, 29 West 39th Street, New York City, at a price of $1.00.

A CORRECTION

In the article by H. Muench on "Discrete Frequency Distributions Arising from Mixtures of Several Single Probability Values" which appeared in this JOURNAL 33 (1938), 390, a correction should be made on page 392. The third line of equation (7a) should read:

$$k = a^{r+1}/r! \text{ where } \Sigma y \text{ is put equal to 1 and } r! = \Gamma(r + 1)$$

STATISTICAL NEWS AND NOTES

Central Statistical Board

The President, by a letter dated May 16, 1938, has requested the Central Statistical Board to report to him not later than January 1, 1939, on the burdens and duplications involved in statistical work carried on by the Federal Government, with particular reference to the numbers of financial and other statistical reports and returns regularly required from business and industry and from private individuals under existing laws.

The Board has appointed a special committee to direct the work on the report. The membership of the committee includes the Chairman of the Board, Stuart A. Rice, as Chairman; Messrs. E. E. Day, President of Cornell University; E. Dana Durand, U. S. Tariff Commissioner; E. A. Goldenweiser, Director of the Division of Research and Statistics of the Board of Governors of the Federal Reserve System; and Morris A. Copeland, Executive Secretary of the Central Statistical Board. Mr. Frederick F. Stephan, Secretary-Treasurer of the American Statistical Association, has been chosen Secretary of the Committee.

The staff of the Central Statistical Board is now engaged in a series of investigations which are directed to securing data which the Special Committee feels that it will need in meeting the President's request for constructive suggestions looking toward a reduction of the number of financial and other statistical reports and returns without handicapping either the Government or business users of information collected by the Government.

Letters have been addressed to the heads of Federal agencies, outlining the Board's needs, and informal conferences have been held to explain the program in more detail and to make arrangements for the submittal to the Board of complete sets of questionnaires and return and report forms used during the last fiscal year, certain descriptive materials about them, and copies of the statutory authorizations, executive orders, regulations and departmental or other orders upon which each is based. Much of the information required has already been collected by the Board in connection with its regular work program, but a comprehensive re-review of all current statistical work has been decided upon in order to supplement and complete the materials available in the Board's files.

Members of the Board's staff have supplied copies of special forms for the listing and cataloging of all questionnaires and other report forms sponsored by each agency. The first of the forms consists of a general check list on which the name of each questionnaire or other report or return will be entered. This list will be keyed by the agencies to individual copies of a second form which will be used as an identification sheet upon which will be entered a description of each form, together with such other remarks as the agency deems pertinent to the special problems which may have arisen in the use of the form. The check list and the identification sheets are being analyzed by the Board's staff and tabulations will be made of the data for use in preparing the final report.

The Technical Committee for Manufacturing Industries has completed its work for the interagency Committee on Industrial Classification (see this JOURNAL, 33 (1938), 235) on a preliminary classification of manufacturing industries. Volume I, Part 1, of the Standard Industrial Classification Code, containing code numbers and titles of manufacturing industries, has been multigraphed and is available for distribution upon request. The Technical Committee has also completed its work of preparing a description of industries and lists of principal products and principal operations. The list of principal products and operations will be presented as a general index for manufacturing industries.

Dominion Bureau of Statistics, Canada

Social Analysis. The Dominion Bureau of Statistics has recently inaugurated a seminar for the purpose of discussing specific problems arising in the course of the work of the various Branches. The persons most concerned prepare and read before the group papers describing specific problems they have encountered and their methods of approach in overcoming them. The ensuing discussions are productive of many helpful suggestions and so far the seminar has proved most successful.

Education. During the week of May 23rd, several national organizations in the field of the social sciences met in Ottawa, and representatives of these, together with Dominion Government officials, met to discuss means of improving and facilitating Canadian research in this field. Two meetings were held, at the first of which the Dominion Statistician addressed the group on the relation of statistics to the social sciences. A discussion followed on the possibility of establishing a committee on social research to which the organizations taking part were asked to name representatives. These organizations were: the Canadian Historical Association, the Royal Society of Canada (Section 2), the Canadian Political Science Association, the Canadian Institute of International Affairs, the Canadian Committee of International Geographical Union, and the Canadian Agricultural Economics Society. The Canadian Psychological Association and the Canadian Education Association were also invited to participate but acceptance has not yet been received. The Chairman of the Committee is Professor Reginald G. Trotter of Queen's University.

External Trade. The primary compilation of trade statistics, formerly carried out by the Department of National Revenue, has been transferred from that Department to the Dominion Bureau of Statistics. In the past the Dominion Bureau of Statistics has had to depend on the Department of National Revenue for the primary data, but in the future will be responsible for the compilation and publication of all trade statistics from their source.

Division of Research and Statistics, U. S. Treasury Department

The Treasury Department is preparing for publication a statistical report containing data compiled from income tax returns, under an allotment from

funds made available to the Works Progress Administration for research projects. The report consists of statistical tables prepared from income tax returns for 1934 of individuals, partnerships, and fiduciaries, and provides information supplementary to that regularly included in the annually published report "Statistics of Income." The project was supervised by the Director of Research and Statistics for the Treasury Department, with the cooperation of the Central Statistical Board and consulting experts from other agencies, and of the Works Progress Administration.

The first section of the report, entitled "Statistics of Income Supplement, compiled from Federal income tax returns of individuals for the income year 1934, Section I," showing number of individual income tax returns for 1934 classified by counties, and cities of 25,000 and over population, by net income classes, was issued during July. In this report the number of returns, Form 1040, designed for individuals with net incomes from salaries or wages of more than $5,000 and incomes from business, professions, rents or sale of property, is shown separately from the number of returns, Form 1040-A, designed for individuals with net incomes of not more than $5,000 derived chiefly from salaries and wages. This publication may be secured from the Superintendent of Documents for a nominal charge. Additional sections of the report will be issued as completed.

In addition to the report which will be published, a detailed set of tables of corporate and individual financial information reported on income tax returns has been compiled. These tables are supplemental to "Statistics of Income" for the years 1926–1933, inclusive, as one part of the project carried on with funds from the Works Progress Administration. Similar tabulations for 1934 and 1935 are in preparation by the Bureau of Internal Revenue, and it is planned that those for subsequent years will be made as rapidly as possible after the data become available.

This Source Book of detailed tables is primarily a transcription for each year from 1926 through 1933 of the work sheets of corporate and individual income tax tabulations and will be available to qualified students. Application to inspect the Source Book should be addressed to the Secretary of the Treasury.

The additional data provided for each year by the Source Book are described below in terms of the tables published in "Statistics of Income for 1933."

The receipt, deduction, and tax items of all corporations (see stub of Table 13) are shown for each of 91 industrial groups in each of the 51 States and Territories for each year from 1926 through 1934. The asset and liability items listed in Table 15 are shown for each year from 1926 through 1929 for each of the 21 major industrial groups, returns with net income and returns with no net income being shown separately. For 1930 this table is shown in summary form for each of the 91 industrial groups, and for 1931 through 1933 it is shown for each of the 91 industrial groups in each State and Territory.

Selected items of assets, liabilities, receipts, deductions, and tax (the

items in the stub of Table 16) are shown by the 21 major industrial groups and 9 size groups (size being measured by total assets) for each year from 1931 through 1933. Corporate returns for each year from 1926 through 1933 are shown by size of net income and deficit classes varying in number according to years, and by 21 major industrial groups, giving the number of returns, net income or deficit, and tax. A special table of consolidated returns shows the items of assets, liabilities, receipts, deductions, and tax listed in Table 16, for 91 industrial groups for 1933.

The most valuable table based on the individual returns shows for each year from 1927 through 1933, for each net income class, by States, data on sources of income and deductions (as in stub of Table 6 of the 1933 volume). A second table shows for each State data on the number of returns and amount of net income classified by size of net income and cross-classified by sex and family relationship (i.e., Table 4 of the 1933 volume is given separately for each State).

Board of Governors of the Federal Reserve System

Statistics of international capital transactions of the United States for the first quarter of 1938 were published in the *Federal Reserve Bulletin* for July 1938.

A series of tabulations showing average operating ratios of member banks, by size groups, in the various geographic divisions and Federal Reserve districts for the year 1937 was published in the August 1938 issue of the *Federal Reserve Bulletin*. The data presented relate to the sources and disposition of earnings, the rates of earnings and losses on loans and on investments, and the rates of earnings and net profits in relation to available funds and to capital account. The ratios were computed by averaging arithmetically the corresponding ratios of individual banks, which were prepared and made available by the Federal Reserve banks.

The Board's seasonally adjusted indexes of factory employment are being revised to take account of revisions in the underlying indexes of the Bureau of Labor Statistics and of changes in the seasonal characteristics of some individual employment series. The revisions in the indexes of the Bureau of Labor Statistics reflect for the most part adjustments to the 1935 Census level. Revised seasonal adjustment factors by industries and seasonally adjusted indexes by groups and by industries are to be published in an early issue of the *Federal Reserve Bulletin*.

The Federal Reserve chart book, prepared for use of Federal Reserve officials, is now available for sale to the general public at a price of 50 cents a copy, which covers the cost of paper and printing. The chart book, which is of brief-case size, contains about 30 charts on bank credit, money rates, security markets, and business conditions. Most of the charts cover the period since 1919 and all contain space for figures through 1940 with a lightly printed grid for 1938–1940 to aid in keeping them up-to-date. A list of sources for all past and current figures is given.

Bureau of Foreign and Domestic Commerce[1]

The retail trade series of the Current Statistical Service is being expanded to include the publication of monthly figures from independent stores in the States of Pennsylvania and Florida. Data for Western Pennsylvania are being secured in cooperation with the Bureau of Business Research, University of Pittsburgh, and a monthly report will be issued for Pittsburgh and the Western Pennsylvania area as well as the report for the state. It is also planned to publish sales changes by kinds of business for the city of Philadelphia as soon as the sample in that area can be built up. The state data will be presented to show sales by kinds of business, by city size groups, and for individual towns and cities wherever the sample is considered adequate.

In connection with studies of the National Economic Committee of which Col. Richard C. Patterson, Jr., Assistant Secretary, is a member, Dr. Willard A. Thorp, Economist for Dun and Bradstreet, Inc., has been appointed as an adviser on economic studies. The facilities of the Bureau of Foreign and Domestic Commerce will be utilized in connection with these studies.

The office of Chief Statistician of the Bureau of Foreign and Domestic Commerce, which has been vacant for some time, is filled by Mr. Ernest A. Tupper. Mr. Tupper was formerly chief of the Division of Foreign Trade Statistics.

A Division of Business Review has been established with Mr. M. Joseph Meehan as Chief. Mr. Meehan was formerly Editor of *The Survey of Current Business*. This Division will publish *The Survey of Current Business*, the weekly supplement thereto, and *Domestic Commerce*.

Bureau of Labor Statistics, U. S. Department of Labor

The Retail Price Division has completed a revision of the fuel price index to show grades and sizes of bituminous coal in more detail than has heretofore been available. The number of dealers reporting has been increased, and the indexes are weighted on the basis of estimated consumption for household heating. Rent indexes by types of dwelling have been released in mimeographed form for 20 cities.

A summary of the expenditure data for 32 cities, obtained through the consumer purchases study made by the Bureau, has been released in a mimeographed report "How Urban Families Spend Their Incomes." The families are grouped at 15 income levels, and data are shown for 2 metropolitan areas, 6 large cities, 6 groups of middle-sized cities, and 2 groups of smaller cities. There will be available in the fall a further analysis of the distribution of families by income, occupation, and family type, and of the distribution of expenditures. Bulletins showing in detail money disbursements of wage earners and lower-salaried clerical workers will also be issued shortly.

[1] See also p. 579.

A study has been completed showing average weekly and hourly earnings and average hours per week separately for full-time and part-time workers in retail trade. The data pertain to New York State and were collected by the New York State Department of Labor. The information is shown for 16 branches of trade and cover the three years 1935–37, by months. Significant differences in hours and earnings between the full-time and part-time employees were disclosed. A conference of statisticians from the 16 state offices which cooperate with the B. L. S. in the collection of employment and payrolls was held in Washington from June 20–22. An account of the conference may be found on page 575 of this JOURNAL.

Special surveys of wages and hours have been undertaken for iron and steel and for the radio transmission apparatus industry. Work is nearing completion on similar surveys of electrical machinery and apparatus, furniture, meat packing, woolen and worsteds, and fertilizer. The study of entrance wage rates of common labor, made annually as of July 1, is in progress.

Production in prison industries is the subject of a special study recently issued. It compares production and conditions of prison labor in 14 states in 1936 with earlier years. Two new surveys of labor requirements have been made, dealing with the sand and gravel, and electrical apparatus and supplies industries.

There is in preparation a manual on industrial injury statistics. It will deal with methods of collecting, coding, analyzing and presenting these data, with particular emphasis on the coding of accident causes in line with the newly developed Heinrich cause code. Standard forms for reporting industrial accidents, suggested office record forms, and the types of tables essential for the use of state administrative bodies dealing with workmen's compensation will be included. The manual is intended to replace B. L. S. Bulletin 276, "Standardization of Industrial Accident Statistics," which was issued in 1920.

Women's Bureau, U. S. Department of Labor

The Women's Bureau of the Department of Labor has just completed nation-wide surveys of wages and hours in the evaporated and condensed milk industry, in cereal plants, and in drug and medicine manufacturing plants. The Bureau is now making an intensive study of labor problems in the canning and dried fruit packing industry to meet several needs: (1) to furnish a basis for development of feasible methods of establishing state minimum wage rates and hours regulations in canneries by state minimum wage commissions; (2) to furnish the Public Contracts Board with 1938 wages and hours conditions within the canning and dried fruit packing industries, as well as with a picture of the seasonal variations in earnings and hours, in order to permit the establishment of minimum prevailing rates for government contracts on an enforceable basis; (3) to permit the administrator of The Fair Labor Standards Act to interpret the Act as it relates to the Canning Industry in accordance with current facts on opera-

tions; and (4) to furnish the Unemployment Compensation Division of the Social Security Board with data concerning the volume of employment during the year in order that equitable adjustments may be made in unemployment compensation regulations.

Bureau of Old-Age Insurance, Social Security Board

The present statistical program of the Bureau of Old-Age Insurance includes three types of statistical studies. The first of these consists of administrative statistics showing the development and progress of the operation of Title II of the Social Security Act; the second consists of vital statistics which are by-products of the record-keeping operations of the Bureau; and the third, wages, pay rolls, and employment statistics which are obtained in the course of keeping records necessary to the payment of benefits.

The most significant of the administrative statistics may be divided into three main categories: (1) statistics on claims and account numbers activities of the field offices of the Bureau; (2) quantitative statistics of record-keeping operations in the Baltimore Office of the Bureau; and (3) statistics on claims operations in the Washington Office of the Bureau.

The current information concerning claims and account numbers activities and other items which are of particular interest to persons outside of the Bureau are included each month in the *Social Security Bulletin.*

In the course of the adjudication of claims and the granting of account numbers to wage earners, certain facts are collected concerning all holders of account numbers, especially the age, sex, and color. A sample study of these characteristics has recently been made, which includes ten per cent of all persons who applied for account numbers up to January 1, 1938, and a complete tabulation of those persons applying for such numbers since the beginning of 1938 is being made monthly. Results for January-April 1938 were published with *Social Security Bulletin* for June 1938.

The second type of information obtained in the operation of the Old-Age Insurance program consists of some of the facts gathered in the adjudication of claims. A sample study has been made to determine the significance and adequacy of this information for statistical purposes, and a punch card has been designed for use in the analysis of the claims filed beginning in 1938. The items to be included in the study are the personal or social characteristics of the wage earners themselves, including occupation, sex, color, age, and number of children; certain facts concerning the heirs or payees, and in the case of death claims last illness and funeral expenses. Periodic tabulation of this material will be made perhaps on a quarterly basis.

In developing an occupational classification, the Bureau is attempting to incorporate its needs into the broader problems of occupational classification being studied by the Committee on Occupational Classification appointed by the Central Statistical Board and the American Statistical Association.

The third type of statistics being developed by the Statistical Section of the Bureau covers earnings and employment data which are available from the reports required by the Treasury in connection with the collection of taxes under Title VIII of the Social Security Act. Present plans for tabulations of these reports for 1937 include: (1) The number of employers making returns, the number of wage reports, the taxable pay roll, by size of the employing organization as measured by the number of employees. These tabulations will be by industry and by State, as well as by other geographical divisions. (2) The number of employees for whom returns are made, the number of wage reports, total taxable earnings by age, sex, and race, and by State. Separate tabulations are being made for the first and second six month periods of 1937, with a summary for the year.

Division of Research, Statistics and Records, Works Progress Administration

Statistical reporting procedures have been revised in order to conform to administrative changes made necessary by the Work Relief and Public Works Appropriation Act of 1938. Under the Emergency Relief Appropriation Acts of 1935, 1936, and 1937, funds were appropriated to the President who made direct allocations to the various Federal agencies participating in the Works Program. Under Title I of the 1938 Act, funds are appropriated by Congress directly to the Works Progress Administration and it is authorized to allocate up to $60,000,000 to other Federal agencies for operation of projects of a type similar to those operated by WPA. Statistical reports covering such Federal agency projects will be secured by the Works Progress Administration in addition to continuation of reports regularly prepared for projects operated directly by the WPA and by the National Youth Administration.

Tabulations of employment and payrolls on other projects financed from Federal funds, including other titles of the Work Relief and Public Works Appropriation Act of 1938, will be compiled by the Division of Construction and Public Employment in the Bureau of Labor Statistics.

Reports of physical accomplishments on WPA projects since October 1, 1937, when the last summary was made, are being prepared for tabulation. The new data will cover all units of work completed on WPA projects placed in operation from the beginning of the program in the summer of 1935 through June 30, 1938.

Studies based on the records formerly maintained by the Area Statistical Offices through November 1937 will be completed by September 1, 1938. A tabulation showing the assigned occupations of workers employed on WPA projects during the month of November 1937 has been completed and is available. A detailed analysis of labor turnover by months from the beginning of the WPA program through November 1937 is being made and data on duration and continuity of WPA employment are being compiled for all states and for major urban areas.

Division of Social Research, Works Progress Administration

Survey of Youth in the Labor Market. This survey will give particular attention to the transition of youth from school to regular productive activity. The work records of these youth will be examined with reference to their school attainments, personal characteristics, and background at the time of entering the labor market. The survey is now operating in seven representative cities and will approximate 25,000 completed schedules.

Analytic Index of Social Statistics. Two years ago the Division of Social Research, Works Progress Administration, established a card catalog of statistics concerning relief and related subjects. This index maintains the major cross tabulations of statistical material by the use of interrelated subject headings which have been classified under more than 800 main categories. Approximately 2,000 reports, published by both public and private agencies, have been analyzed and indexed. The Index is available for reference in Washington to research workers and other persons who are interested in relief and related subjects.

Relief Needs and Employment Possibilities. A survey to determine the adequacy of current relief programs, the reemployment possibilities of Works Progress Administration workers and the absorption of these workers by private industry was recently made in nine of the cities which had been similarly surveyed during the summer and fall of 1937. This survey showed that there is considerable local diversity in relief policies ranging from definitely unsatisfactory to fairly adequate.

Cultural Regions Within the Rural-Farm Population. The delineation of socio-economic regions and sub-regions for the rural-farm population of the United States based on various pertinent indices has been completed and twenty-nine major regions containing 210 sub-regions have been delimited and mapped. A mimeographed statement of methodology has been prepared. Ultimately, cultural sub-regions within the rural-nonfarm population will also be delineated. It is planned to unite these two types of groupings to obtain regions of relative homogeneity with respect to the combined rural-farm and rural-nonfarm population. The Urban Section of the Division of Social Research is assembling material for the preparation of a similar memorandum for the classification of cities.

Study of Labor Changes and Relief in the Cotton Belt. A survey of the operations of cotton plantations is being conducted in Arkansas, Georgia, Louisiana, Mississippi, North Carolina, and Texas. Plantations in the first five of these States were previously studied in 1934 and reported in "Landlord and Tenant on the Cotton Plantation" by T. J. Woofter, Jr. The purposes of this survey are to analyze the changes which have taken place in the relief situation since 1934. The trend in mechanization and developments in cropping and other farm enterprises are also being studied with respect to their effect on the availability and utilization of labor.

Graduate School of the Department of Agriculture

The results of the deliberations of a committee on the unification of the mathematics and statistics courses have been published in the 1938–39 announcement of courses given by the Graduate School of the Department of Agriculture. All mathematics courses given have as their aim the preparation of the student for some branch of statistics. A course in algebra is taught by Harry J. Winslow as a prerequisite of the "Introduction to Statistical Analysis" given by B. R. Stauber and G. E. Ockey. Following this elementary course directly are: "Statistical Laboratory" (Dr. B. Aitchison), "Index Numbers and Time Series Analysis," "Correlation Analysis" (C. M. Purves), and "Statistical Treatment of Experiments in the Plant and Animal Industries" (Dr. A. E. Brandt). In addition to algebra and some statistics, "Trigonometry and Geometry" (Harry J. Winslow) is required before taking Alexander Sturges' course in "Interpretation of Statistical Calculations."

Then follows a series of more advanced courses in statistics. Calculus (given alternate years) is a prerequisite for Dr. W. E. Deming's course in "Least Squares," for which the text will be his book under the same title recently published by the Graduate School. He also gives "Calculus of Finite Differences." Two courses are given by Mr. M. A. Girshick; the first is "Linear Algebra" which can be followed by "Multivariate Analysis." Dr. Max Sasuly offers two unusual courses in advanced statistics: "Analytic Curve Fitting" and "Dynamic Analysis of Time Series." "Higher Algebra" (Dr. E. A. Rasor and D. C. Bronson), and "Advanced Calculus" (A. Wertheimer) supplement the previously given mathematics courses in building a foundation for courses in statistics and statistical theory. "Special Topics in Statistics" will be a series of lectures given by outstanding specialists in their particular fields.

Five lectures on "Some Relations between Certain Continued Fractions, Orthogonal Polynomials and Stieltjes Integrals" were given before the statistics seminar this spring by Dr. C. Winston. The plan is to have these lectures hectographed, thus making 60 or 70 copies available. The lectures given by Dr. Walter A. Shewhart of the Bell Telephone Laboratories under the sponsorship of the Graduate School will also be published soon.

The Civil Service Commission informed the Department of Agriculture that the Commission will accept direct certification of those courses given by the Graduate School that are of graduate or college grade. This procedure will benefit those who desire recognition for credit courses in the Graduate School in connection with the Civil Service examinations.

Brookings Institution

The Brookings Institution has under way a study of the organization and functioning of the system of odd-lot trading used on the New York Stock Exchange. The study is in charge of Dr. Charles O. Hardy. The lead-

ing odd-lot houses are cooperating in furnishing statistical information for this study.

The American capital market, its nature and functions, and its relation to the country's economic system as a whole is the subject of a group project now in progress at the Institution.

Research Projects at Dun & Bradstreet, Inc.

Mid-Year Inventory Survey. The Business Trend Survey announced in the March issue of this JOURNAL, and described in some detail in the June issue, proves to be of such wide interest that it was decided to bring the inventory portion of the study up to date as of June 30th.

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Graduate School of the Department of Agriculture

The results of the deliberations of a committee on the unification of the mathematics and statistics courses have been published in the 1938–39 announcement of courses given by the Graduate School of the Department of Agriculture. All mathematics courses given have as their aim the preparation of the student for some branch of statistics. A course in algebra is taught by Harry J. Winslow as a prerequisite of the "Introduction to Statistical Analysis" given by B. R. Stauber and G. E. Ockey. Following this elementary course directly are: "Statistical Laboratory" (Dr. B. Aitchison), "Index Numbers and Time Series Analysis," "Correlation Analysis" (C. M. Purves), and "Statistical Treatment of Experiments in the Plant and Animal Industries" (Dr. A. E. Brandt). In addition to algebra and some statistics, "Trigonometry and Geometry" (Harry J. Winslow) is required before taking Alexander Sturges' course in "Interpretation of Statistical Calculations."

Then follows a series of more advanced courses in statistics. Calculus (given alternate years) is a prerequisite for Dr. W. E. Deming's course in "Least Squares," for which the text will be his book under the same title recently published by the Graduate School. He also gives "Calculus of Finite Differences." Two courses are given by Mr. M. A. Girshick; the first is "Linear Algebra" which can be followed by "Multivariate Analysis." Dr. Max Sasuly offers two unusual courses in advanced statistics: "Analytic Curve Fitting" and "Dynamic Analysis of Time Series." "Higher Algebra" (Dr. E. A. Rasor and D. C. Bronson), and "Advanced Calculus" (A. Wertheimer) supplement the previously given mathematics courses in building a foundation for courses in statistics and statistical theory. "Special Topics in Statistics" will be a series of lectures given by outstanding specialists in their particular fields.

Five lectures on "Some Relations between Certain Continued Fractions, Orthogonal Polynomials and Stieltjes Integrals" were given before the statistics seminar this spring by Dr. C. Winston. The plan is to have these lectures hectographed, thus making 60 or 70 copies available. The lectures given by Dr. Walter A. Shewhart of the Bell Telephone Laboratories under the sponsorship of the Graduate School will also be published soon.

The Civil Service Commission informed the Department of Agriculture that the Commission will accept direct certification of those courses given by the Graduate School that are of graduate or college grade. This procedure will benefit those who desire recognition for credit courses in the Graduate School in connection with the Civil Service examinations.

Brookings Institution

The Brookings Institution has under way a study of the organization and functioning of the system of odd-lot trading used on the New York Stock Exchange. The study is in charge of Dr. Charles O. Hardy. The lead-

ing odd-lot houses are cooperating in furnishing statistical information for this study.

The American capital market, its nature and functions, and its relation to the country's economic system as a whole is the subject of a group project now in progress at the Institution.

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CHAPTER ACTIVITIES

The Albany Chapter

A dinner meeting was held on April 26, with 34 members and guests present. The speaker was Dr. Arthur W. Bray, Professor of Biology, Rensselaer Polytechnic Institute, Troy, New York. He gave an excellent philosophical discussion on "Probability and Its Implications in Everyday Life." Dr. Bray's talk was extremely stimulating in that it attempted to tie up underlying philosophies with the more purely statistical concepts of the present time. There was lively discussion from the floor.

Dr. W. W. Coxe gave a progress report on the work of the Committee on Professional Standards for Statistical Workers, of which he is Chairman. This Committee is working on the problem of securing more satisfactory salary allocations for the statistical workers in the service of New York State than is provided for in the present law governing allocations of statistical workers to salary grades.

On May 10, a dinner meeting was held at which 29 members and guests were present. The speaker was Mr. R. E. Wareham, General Electric Company, Schenectady, New York, on the subject, "Statistical Technique as a Tool for Industrial Control." Mr. Wareham stated that the application of statistical methods in mass production industries provides a means of controlling variations in quality. The steps in setting up this control are as follows: (1) determine the actual quality characteristics of the product; (2) show what variations in quality are permissible as "chance causes"; and (3) if these chance limits are exceeded, locate and correct the "assignable cause."

He further stated it may be recognized that in the field of large scale production even a small reduction in cost means substantial savings to business. These savings are passed on to the consumer in the form of lower selling prices. Quality-control technique provides tools for reducing these costs and for improving quality. Mr. Wareham discussed a specific case showing the application of statistical methods and tools.

At this meeting the following officers were elected for the next year: President, F. J. Decker; Vice-President, C. M. Armstrong; Secretary-Treasurer, Miss Mildred Lauder; and Directors, E. H. Van Winkle and Roland F. Bucknam.

The Austin Chapter

The Austin Chapter held its fourth and final regular meeting of the academic year 1937–38 on May 5, 1938. The main address was delivered by Mr. Garth Daniel, Statistician, Texas Old Age Assistance Commission, who reviewed the work of the statistical department of that organization and described generally the methods of procedure adopted by the Commission. Mr. Daniel pointed out several projects of a statistical nature which were being planned and described some of the obstacles involved. A considerable portion of his time was taken in answering questions put by his listeners,

the meeting concluding with a general round table discussion of the subject of old age assistance.

Officers of the Chapter for the ensuing year were elected as follows: President, Dr. Edward L. Dodd; Vice-President, Mr. H. G. Heard; Secretary, Mr. A. P. Vickery. An executive committee consisting of the retiring officers and the incoming officers was authorized to function as a coordinating agency in planning the activities of the Chapter for the coming year.

The Boston Chapter

A special meeting of the Boston Chapter was held at the Massachusetts Institute of Technology on Thursday evening, June 16, 1938. There were 46 members and guests present. After the dinner a short business meeting was held, at which Mr. Edmund S. Cogswell, First Deputy Commissioner of the Massachusetts Department of Insurance, was elected President of the Chapter.

Dr. Carl Snyder, for many years with the Federal Reserve Bank of New York, discussed the subject: "High Profits and High Wages." His address was illustrated by numerous charts, displayed by lantern slides. A summary of his address, reported by Dr. Wilfred S. Lake, follows:

"Why is the United States so rich? Abundant natural resources is not the answer, because Mexico, our next door neighbor, has great resources but is not wealthy, and has a wage level about one-tenth that of the United States. The basic explanation is simple: wealth comes from the supply of capital and will be greatest and will increase most rapidly where there is the most capital. Further, the greatest wealth is found where there is the greatest concentration of wealth.

"This thesis is substantiated by the statistical evidence afforded by the graphic comparison of numerous relevant series. Particularly significant is the striking correspondence between the curves representing the increase of production and the increase of capital invested in manufacturing. It appears that for every billion dollars of new product we must invest about $1\frac{1}{4}$ billion of new capital.

"Why are our wages so phenomenally high, the highest ever known? The answer lies in the relation between wages, national income, and capital accumulation. An analysis of the division of income in all manufacturing since 1849 reveals that the share of wages, aside from the early years when capital was being added very rapidly, has been nearly constant. Even such an industry as clothing, which became unionized in 1899, exhibited only a slight increase in the wage percentage after that date. Furthermore, it should be noted that total dividend and interest shares have usually been small, between 5 and 8 per cent.

"When we turn to comparisons since 1850 of wage and salary payments, based upon the index of the Federal Reserve Bank of New York, with various pertinent series, we discover the clue to our problem of high wages in the close similarity between wages, capital invested, and horse power per wage earner. Hence, we may say that the increase in mechanical equipment

is the sole way to increase wages. Wages seem to be a functional part of the system and have not been affected by the growth of unions nor by much ado about monopolies. Indeed, to calculate wages we merely need to know the rate of growth of production, the total product, and the price level.

"Whence came this great increase in capital? Look at the curves for industrial production and industrial profits. They move together, closely, when both are expressed in standard deviation units. Industrial profits, according to statistics on the sources of income for 1923, 1926, and 1929, go largely to the wealthy, as dividends and capital gains. Since only 60 per cent of the 500,000 corporations in the country paid dividends even in 1929, yet accounted for 90 per cent of the total volume of corporate business, the working control of industry is in the hands of the wealthy, those with incomes of $40,000 or over. They are the owners of industry, less than 100,000 people, or some 60,000 families. The comparison of new corporate issues with dividends, net profits, and gross income gives added testimony to the statement that capital emerges primarily from profits. It is doubtful whether the worker contributes over 3 per cent of the total capital invested in industry.

"To conclude: wages are a fixed part of the national income, apparently not greatly altered in a century. National income equals national production in dollars, and national production is due to mechanical equipment or horse power employed, in fact, is almost a function of capital supplied or invested. In all industry production increases *pari passu* with capital investment, and in no other way. Since wages increase only when income increases, all the unions on earth, and all efforts to raise wages by law are like commanding the clouds to change their shapes. If, then, we can raise wages solely through increasing income, one way only is open—increase of capital investment. The law is inexorable: eliminate profits and there can be no increase in capital, and therefore no increase in wages. High wages are a simple function of high profits."

The Chicago Chapter

The sixth and last meeting for 1937–38 of the Chicago Chapter was held May 16th. Officers for the 1938–39 season were elected as follows: President, Henry B. Arthur, Swift & Company; Vice-President, Theodore O. Yntema, University of Chicago; Secretary-Treasurer, Miss Frances Hay, Standard Oil Company (Ind.); and Directors, John H. Noble, Armour and Company; Harold Davis, Northwestern University; and Donald W. Hansen, Illinois Bell Telephone Company.

The speaker of the evening was Dr. Jacob Viner, Professor of Economics at the University of Chicago and recently Special Assistant to the Secretary of the Treasury, Washington, D. C. Dr. Viner discussed criteria and problems of monetary control.

The Chicago Chapter has had a very active season, with an increase in membership of 25 per cent and an average attendance at meetings of 74.

The Cleveland Chapter

The eleventh annual meeting of the Cleveland Chapter was held at the Cleveland Chamber of Commerce Club on May 12, 1938. Forty-two members and guests were present. Dr. Carlton presided.

Mr. L. M. Whittington, Chairman of the Nominating Committee presented the following nominations for officers for 1938–39: Mr. John W. Love, President; Mrs. Frida F. Selbert, Vice-President; Mr. E. A. Stephen, Secretary and Treasurer. No other nominations were offered and the nominees were unanimously elected.

Three papers were presented at the meeting. Dr. L. Dewey Anderson, Professor of Psychology at Western Reserve University, spoke on the subject, "Motor Skills." The address concerned the results of tests of physical endurance and skill subjected to children of both sexes in the Cleveland schools. About 4,000 children submitted to these tests. Schools were selected for the tests where there were pupils who represented a good sample of Cleveland's population as to race and economic status. The talk was illustrated with a variety of wall charts. In general, the tests showed that the boys were superior to the girls in both strength and skill. The average strength and skill of the 18-year olds were used as a measure of maturity. A high degree of maturity was reached at varying ages, depending upon the type of test conducted. The tests revealed that the training for sports should begin at an age depending upon the age at which the majority reach maturity. In general, the athletes scored higher than the non-athletes.

Mr. Tell Berna, Manager of the National Machine Tool Builders' Association, spoke on "Tools of Industry." The talk concerned the growth and importance of the machine tool industry. Lantern slides were used to illustrate the address. Mr. Berna traced the development of the industry since 1760 when Watt developed a machine to construct the steam engine. The size of the average machine tool plant is small as is the total industry. Management is conservative. The industry requires a high degree of skill in its labor and a man of advanced years is an asset to the industry. Because the volume of business is highly fluctuating, it is difficult to keep a crew together since a good machine tool man is highly desirable in other industries. There are usually three poor years to two good ones. While there are violent fluctuations in orders, the manufacturing activity is not quite as violent since the companies operate with large backlogs of orders in prosperous times. At the present time, the smaller customers are more reluctant to buy than the larger ones. About 25 per cent of all machine tools are sold to automotive customers, 40 per cent including the accessory companies.

A comparison of the index of orders for machine tools with the Reserve Board's index of industrial production revealed that both indexes decline at about the same time but that the machine tool index lags on the upturn. This may be due to exhaustion of cash. Wage rates may be favorable and costs are low. New tools are not needed so badly. The index of machine tool orders is no basis for a forecast of industrial activity.

Mr. Berna discussed the increase in efficiency made possible by the use of modern machine tools. He stated that new machinery does not usually create unemployment. A chart which compared population growth with the number of persons employed revealed a faster growth in employment than in population. Another slide compared the decrease since 1899 in the hours of work in factories with the increase in hourly wage rates.

Mr. Howard W. Green, Director of the Real Property Inventory of Metropolitan Cleveland, gave a talk on the "Development of Cleveland during the Past Century." An interesting series of maps were presented which showed the number of lots created by five year intervals. The largest amount of activity took place from 1870 to 1874 when 28,000 lots were created. Another series of maps showed the lots created on a cumulative basis. This study will be of considerable value in forecasting the areas of future growth in sub-lots, building, and population.

The Connecticut Chapter

A meeting of the Connecticut Chapter was held on April 25. Mr. W. R. Williamson, Actuarial Consultant of the Social Security Board, Washington, D. C., spoke on "Some Phases of Unemployment Benefits." On June 6, Professor Gunnar Myrdal spoke on "Housing Problems and Housing Policy in Sweden" at a joint meeting of the Connecticut Chapter and the Yale Economics Club. Dr. Myrdal is Professor of Political Economy and Director of the Social Science Institute at Stockholm University and is a member of the Swedish Parliament. He has been adviser to the Social-Democratic government on population, housing, and finance.

The Madison, Wisconsin Chapter

The group interested in the formation of a local chapter of the American Statistical Association in Madison, Wisconsin, held preliminary organization meetings on April 1 and April 8. The proposed charter and by-laws were drafted and submitted by the sixteen local members of the American Statistical Association to the parent body for approval, and a charter was granted late in April.

Subsequently the following officers were elected: President, Professor Harry Jerome of the Department of Economics, University of Wisconsin; Secretary-Treasurer, Professor Philip G. Fox of the School of Commerce, University of Wisconsin; and Vice-Presidents, Professor W. P. Mortenson of the Department of Agricultural Economics, University of Wisconsin, Mr. Claude S. Holloway of the research staff of the Wisconsin Taxpayers Alliance, and Dr. Walter H. Ebling, Chief, Division of Agricultural Statistics, Wisconsin Department of Agriculture and Markets. The membership of the chapter numbers about thirty-five and consists chiefly of three groups: persons associated with the University of Wisconsin, statisticians with various departments of the state and federal government in Madison, and statisticians associated with business and industry. The three Vice-Presidents are designated as representing respectively these three groups.

The first program meeting of the chapter was held on May 2, with an attendance of about forty. Dr. Oskar Morgenstern, Director of the Österreichisches Institut für Konjunkturforschung at Vienna, prior to the annexation of Austria by Germany, explained the activities of the Institut with special emphasis on the development of cooperation between his organization and similar bureaus in other European countries. At a program meeting on May 18, the chapter was favored with a discussion by one of the Association's Vice-Presidents, Dr. Halbert L. Dunn, Chief of the Vital Statistics Division of the Census Bureau. Dr. Dunn discussed some of the special problems met in conducting the work of the Bureau, particularly the problem of maintaining comparability and at the same time improving the precision of definition and the scope of the data collected.

The Pittsburgh Chapter

Wilbert G. Fritz, Statistician and Financial Economist, Bureau of Business Research, University of Pittsburgh, discussed the "Index of Business Activity in the Pittsburgh District, 1884–1937," before a meeting of the Pittsburgh Chapter on April 28. At the meeting on May 26, Charles A. Carpenter, District Manager of the U. S. Department of Commerce, discussed "The Use of Specialization as Against Generalization in Attempting to Forecast Trends." The June meeting was a joint meeting with the Chamber of Commerce and the Kiwanis Club to hear Walter A. Jones describe "The New All Weather Turnpike Across the State." The subject of the July meeting was "Changes in Steel Prices and Elimination of Pittsburgh as the Basing Point." R. L. Hartford, local representative of "Steel" and Glenn E. McLaughlin, Bureau of Business Research, University of Pittsburgh, initiated the discussion of these recent changes and their probable effect on the steel industry and the Pittsburgh industrial district.

The San Francisco Chapter

Professor George F. McEwen, of the Scripps Institution of Oceanography, spoke at a meeting of the San Francisco Chapter on April 21 on "Some Statistical Problems Arising in Long-time Weather Forecasting in California." He pointed out that an accurate determination of the relationship between seasonal weather and conditions in the ocean is highly necessary, for example, in the kelp industry. Kelp is a commodity of great commercial value which is harvested seasonally. If temperatures are high, it is difficult to harvest and is in poor condition. It is possible to project monthly averages of daily observations of surface temperatures on the assumption that previously observed conditions will determine temperatures several months in advance. In general, three months is about as far in advance as a forecast can be made to be of sufficient reliability for practical use. For precipitation forecasts, observations at six low altitude stations in Southern California have been smoothed to find cyclical relationships.

Hydraulic engineers are confronted with a great variety of problems, the question of flood control, the question of designing of drainage systems,

dealing with long-time projects—dams, etc. The knowledge of the relationship between ocean temperatures and seasonal run-off of rivers is highly valuable in determining what type of structure will be necessary to take care of run-off of rivers from year to year. Since it has been established that surface ocean temperatures have some correlation with the run-off of rivers as well as with precipitation, it is possible by means of this relation and cycle studies to estimate the run-off of rivers a year in advance. Another type of forecasting needed by engineers pertains to estimating, from short series of records, the probable extreme values to be expected in a long interval, fifty or one hundred years.

Dr. Vergil D. Reed, Assistant Director of the U. S. Bureau of the Census, spoke before a joint meeting of the San Francisco Chapters of the American Statistical Association and the American Marketing Association on July 18. His subject was "Needs and Recent Developments in Marketing Research Statistics—Government and Private."

The Washington Statistical Society

A meeting of the Washington Statistical Society was held at the Cosmos Club on April 26 to consider the question "Are We Progressing Toward a Standardized Concept of National Income?" The chairman of the meeting was A. D. H. Kaplan of the U. S. Bureau of Labor Statistics. Robert G. Martin, of the National Industrial Conference Board; Maurice Leven, of Brookings Institution; and Hildegarde Kneeland, of the National Resources Committee, were the principal speakers.

The following officers were elected for 1938–39: President, Halbert L. Dunn, Bureau of the Census; Vice-President, O. E. Kiessling, Bureau of Mines; and Secretary-Treasurer, Thomas B. Rhodes, Works Progress Administration.

Professor Gunnar Myrdal addressed the Washington Statistical Society and the Washington Chapter of the American Sociological Society on "Social and Economic Planning in Sweden" at their joint meeting on May 31.

NEW MEMBERS

Anderson, Professor Don S., Associate Professor, Department of Agricultural Economics, University of Wisconsin, Madison, Wisconsin

Anderson, Paul E., Associate to Consultant Engineer and Economic Adviser, United Mine Workers of America, Washington, D. C.

Aylesworth, Dr. Evelyn, Supervising Statistician, School of Forestry and California Forest and Range Experiment Station, University of California, Berkeley, California

Borwick, Peter M., Assistant Statistician, U. S. Housing Authority, Washington, D. C.

Bottcher, William J., Junior Agricultural Statistician, Bureau of Statistics, New York State Department of Agriculture and Markets, Albany, New York

Brown, Dr. Bonnar, Special Assistant, Division of Security Loans, Board of Governors of the Federal Reserve System, Washington, D. C.

Brown, Daniel K., Investigator, Board of Equalization, State of California, Santa Ana, California

Carmichael, Professor Dan J., Assistant Professor of Business Administration, Drake University, Des Moines, Iowa

Cassen, Philip N., Student, Brooklyn College, Brooklyn, New York

Cogan, Isaac, Statistician, Wisconsin Public Welfare Department, Madison, Wisconsin

Dorman, Robert O., Research Assistant, Unemployment Compensation Division, Connecticut State Labor Department, Hartford, Connecticut

Dowling, Edgar J., Statistician and Market Analyst, Diamond Alkali Company, Pittsburgh, Pennsylvania

Edwards, Wesley, 5189 Raymond Avenue, St. Louis, Missouri

Eisner, Ruth, Associate Statistician, National Research Project, Works Progress Administration, Philadelphia, Pennsylvania

Elder, Jeannette M., Director of Special Studies, Iowa Emergency Relief Administration, Des Moines, Iowa

Epstein, Lenore A., Statistician, Urban Study of Consumer Purchases, U. S. Bureau of Labor Statistics, Washington, D. C.

Field, Maxwell, Acting Secretary, New England Shoe and Leather Association, 210 Lincoln Street, Boston, Massachusetts

Fitzsimmons, Helen P., Provincial Statistician, Department of Federal and Municipal Relations, Government of Province of New Brunswick, Fredericton, New Brunswick, Canada

Foster, Merle A., Agricultural Economist, Marketing Section, Agricultural Adjustment Administration, Washington, D. C.

Geiger, Laverne David, Assistant Actuary, The Industrial Commission of Ohio, Columbus, Ohio

Giffey, Richard C., State Statistician, Michigan Works Progress Administration, Lansing, Michigan

Gordon, Robert Dean, Research Assistant, Scripps Institution of Oceanography, La Jolla, California

Green, Morris M., Jr., Security Analyst, Shearson, Hammill and Company, New York City

Grove, Dr. Robert D., Junior Social Science Analyst, Division of Vital Statistics, U. S. Bureau of the Census, Washington, D. C.

Haskell, A. Porter, Jr., Economist, National Research Project of Works Progress Administration, U. S. Bureau of Mines, Washington, D. C.

Hermberg, Paul G., Asesor tecnico del Contralor General de la Republica de Colombia, Contraloria General de la Republica, Colombia, South America

Hoover, Mrs. Ethel D., Assistant Chief, Retail Price Division, Bureau of Labor Statistics, Washington, D. C.

Keim, Walter G., Associate Statistician, Division of Research, Statistics and Records, Works Progress Administration, Washington, D. C.

Klauber, Laurence M., Vice-President in Charge of Operation, San Diego Consolidated Gas and Electric Company, San Diego, California

Lloyd, Dr. Edward L., Chief, Market Data Section, Bureau of Foreign and Domestic Commerce, Department of Commerce, Washington, D. C.

Long, Glenn S., Head of Examining Division, Ohio State Employment Service, Cincinnati, Ohio

McCarthy, Michael D., Lecturer in Mathematics, National University of Ireland, University College, Cork, Ireland

McInerny, William P., Administrative Assistant, Division of Publications, General Information, Records and Religious Statistics, Bureau of the Census, Washington, D. C.

Mitchell, S. Robert, Assistant Research Analyst, Division of Social Research, Works Progress Administration, Washington, D. C.

Parsons, Charles G., State Statistician, Works Progress Administration, Des Moines, Iowa

Peek, Jesse H., Manager, Investment Department, United States Fidelity and Guaranty Company, Baltimore, Maryland

Phillips, Robert Y., Assistant Coordinator of Statistical Projects, Works Progress Administration, New York City

Platt, C. Spencer, Assistant Manager, Social Security Board, Hartford, Connecticut

Reed, Elizabeth, Assistant Statistician, Bureau of Pneumonia Control, New York State Department of Health, Albany, New York

Reeve, Mrs. Virginia D., Student, Utah State Agricultural College, Logan, Utah

Rogers, H. Gordon, 479 Ridge Street, Newark, New Jersey

Short, C. M., Assistant Secretary, The Canadian Bank of Commerce, West Toronto, Canada

Wendt, Laura, Associate Statistician, Bureau of Research and Statistics, Social Security Board, Washington, D. C.

Wester, Samuel O., Administrative Assistant, Area Statistical Office, Works Progress Administration, Dallas, Texas

Winston, Dr. Ellen, Senior Research Supervisor, Division of Social Research, Works Progress Administration, Washington, D. C.

Winston, Professor Sanford, Professor of Sociology, State College, University of North Carolina, Raleigh, North Carolina

Yaworski, Nicholas, Mineral Economist, National Research Project, Works Progress Administration, Washington, D. C.

C. LUTHER FRY

In Rochester on April 12, 1938, the career of C. Luther Fry was terminated. Born in Philadelphia on March 16, 1894, he received his A. B. degree from Muhlenberg College in 1916, his A. M. from Columbia in 1917, and his Ph. D. from the latter institution in 1924.

The major portion of Luther Fry's life was devoted to social research, and his contributions have been many and significant. Following the World War, in which he served as a commissioned officer, he entered the field of social investigation through his affiliations with the Interchurch World Movement and its successor, the Institute for Social and Religious Research.

A lover of travel, he eagerly seized opportunities for the study of social and economic conditions in the Near East and in the Orient. On numerous occasions he was borrowed for important investigations in various parts of the world. Keenly interested in situations within our own borders, he made significant contributions, both to our knowledge of social and religious phenomena and to the techniques of social investigation.

In 1933 he was called to the chairmanship of the newly created Department of Sociology at the University of Rochester, in which capacity he served until his death. In the City of Rochester the research program which he had built had already yielded valuable results and promised even greater success in the future.

Fry was an indefatigable worker. A list of his published works is too voluminous to quote. Each document was based upon the most painstaking study; each was a distinct contribution.

As a member of the American Statistical Association he was always happy to lend his services to improving social data and to making available unpublished masses of material as well as to the critical analysis of procedures and methods of approach.

His death brings serious loss to the statistical fraternity. It will be hard to find a replacement for him in his teaching and research in Rochester. He was, however, not merely a scientist, a delver in the dusty data of the past. He was a joyous personality who will linger long in the thoughts of his many friends, both colleagues and students.

FRANK A. ROSS

NOMINATING COMMITTEE

In accordance with the provisions of the Constitution, President R. H. Coats has appointed a Nominating Committee to make nominations for officers to be elected at the next Annual Meeting. The members of the Committee are W. Randolph Burgess, Chairman, E. Dana Durand, and Frederick C. Mills.

FREDERICK F. STEPHAN, *Secretary*

BOOK REVIEWS

RALPH J. WATKINS
Review Editor

Industrial Reconstruction and the Control of Competition; The British Experiments, by Arthur F. Lucas. New York, London, and Toronto: Longmans, Green and Company. 1937. xi, 384 pp. $5.75.

Since 1930 British industry has become increasingly involved, both from within and by government imposition, in schemes for the control of prices and production. Today, practically every industry in Great Britain, whose products and processes will permit such control, is definitely contemplating or operating under a minimum price and (less frequently) output limitation and allocation arrangement. It has remained for an American to present the best —in fact, practically the only—complete survey of the astonishing development which has transformed the fundamental character of British economy[1] within the present decade. Professor Lucas follows three introductory chapters on the background and present status of British industrial organization with separate chapters devoted, respectively, to coal mining, iron and steel, shipbuilding and flourmilling, cotton textiles, chemicals and other "combines," agriculture, and an array of industries under the heading "The Trade Association as a Medium of Control." His concluding chapters are devoted to an analysis, in the light of British experience, of the basic issues of control: the determination of prices, regulation of output, maintenance of efficiency, and economic and legal safeguards. The recital of facts in this excellent study will surprise Britons as well as Americans, and the pointed analysis and discussion are likely to result in some hasty stock-taking in the ranks of those who have been lending unthinking support to all phases of the British brand of "rationalization."

Professor Lucas finds that England's movement toward industrial government represents a "deliberate attempt to devise a new and improved industrial structure," growing out of the "near collapse of the British economic system" consequent upon the two post-war depressions and the mounting inability of British industry fundamentally to retain its earlier preeminent place in world economy. Complete loss of confidence in "classical" competition, a need for the rehabilitation of industrial organization and equipment that could be had only through the medium of high centralization, and growing faith generally in co-operative endeavor have all found expression in the new order.

The chapter on coal mining is one of the best in the book—the description of the organization and analysis of the operation and effects of government-re-enforced cartelization, with its rigorous regulation of prices and over-all and individual output, in an industry notorious for its independent individ-

[1] The British *Economist* now talks of the "New Feudalism" (Vol. CXXXI, page 2 (April 2, 1938)); and Sir Arthur Salter, writing on the Coal Bill in the *London Times* of February 9, 1938, says, ". . . we are, by successive Acts, with regard to agriculture, electricity, and other industries, building up a completely new economic system in this country."

ualism. It is to be regretted that the author's treatment had to stop short of the introduction of central selling and the devices contemplated in the legislation enacted by the recent Parliament. The other descriptive chapters are accurate and, in the main, quite adequate, but they lack some of the "feel" and "atmosphere" which might have made them quite distinctive if Professor Lucas had only garnished the material from printed reports with the first-hand impressions which he is in an excellent position to supply. They are good, but they seem, somehow, remote from day-to-day London, Birmingham, Sheffield, and Lancashire.

The author's critical analysis is excellent; he raises questions which the British are all too inclined to ignore—questions which will take their toll if they are not faced and dealt with, a toll that will be none the less real because it is basic and slow rather than immediately and spectacularly devastating. He does well to stress fundamental issues stemming from the central problem of adjustment in an economy of scarce resources; and his point that effective competition will not flow from *laissez-faire* is one that should be brought home to present-day English liberals.

This is a first-rate book. It is informed, well written and effectively organized; the author moves easily and with understanding. If one were to offer any criticism it would be, principally, on the score that the treatment is sometimes too formal and detached, that the author has sometimes seen studied movements where only drifting is present, that "consistent" evidence is occasionally offered as "conclusive," and that (on a matter of detail), in dealing with the law of restraint of trade the author does not distinguish emphatically or early enough between breach of contract (ancillary and independent agreements), tort, and criminal cases. The study runs back over the past fifteen or twenty years; in my judgment, the decisive break came in 1932 with the passage of the Import Duties Act and the institution of the Import Duties Advisory Committee and, thereafter, with the determined establishment of the Government's defense program—affording a necessary condition and tremendous driving force. Things have moved (or "drifted") rapidly in Great Britain since the author's last printed reference (March, 1936) but entirely in the direction which he points. One important fact has become increasingly apparent, however, by the experience of the past two years: the Government, despite its protests and misgivings and over any opposition which may be offered by industry, is inevitably and irresistibly drawn into the very texture of "industrial government" just as soon as formal competition comes to be supplanted by effective cooperation.

BEN W. LEWIS

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Diminishing Returns and Planned Economy, by George M. Peterson. New York: The Ronald Press Company. 1937. xii, 254 pp. $3.00.

It is a little difficult to see why the term "planned economy" should have been attached to the title of this volume unless for the purpose of shifting

the demand curve for it to the right. The discussion of economic planning is limited to a definition and a few observations in the last chapter and to a six-page appendix called "Suggestions for Social Economic Planning in the United States."

The substance of the book is concerned with a short discussion of pedagogical methods in the presentation of economics and with a lengthy consideration of the "most important principle underlying economic relations," namely "the law of diminishing returns." The remarks on pedagogy are interesting and the analysis of diminishing returns lucid and clear-cut. The author is of the opinion that instruction in economics should proceed from simple relationships to complex and likens the presently favored institutional approach to "beginning the study of chemistry with an analysis of fertilizers, vitamins, cellulose, proteins and gunpowder."

The term "diminishing returns" is used broadly enough to cover a discussion of increasing and decreasing average, marginal and total returns in terms of physical product, costs, values, and profits. The first half of the book, however, is devoted to variations in the physical relations of two factors of production and associated variations in the physical product. Although this analysis adds little that is new, it is careful and comprehensive. Most of the remainder of the volume is concerned with the application of the principles of "diminishing returns," thus understood, to economic theory and to economic and sociological problems. Here, partly by reason of the subject matter, but also because of the number and comprehensiveness of the problems the author attempts to cover, the treatment is much less rigorous and tends even to become sketchy. The book as a whole exhibits a curious lack of cohesion and consequently does less than justice to the author's obviously considerable powers of analysis.

EDWARD S. MASON

Harvard University

The Economic Library of Jacob H. Hollander, Ph.D., Compiled by Elsie A. G. Marsh. Baltimore: J. H. Furst Company. (Privately Printed) 1937. xi, 324 pp.

To review Professor Hollander's *Economic Library* in the customary formal manner would be both inappropriate and ungracious. For this handsome volume, printed and bound in the style of Ricardo's day, is much more than a catalogue of books, portraits, and autographed letters; it is a record of a long and pleasant adventure published so that other economists might vicariously share the joys of this happy adventure and profit from the fruit of Professor Hollander's harvest. No idle hope this. Here is our common heritage, 4,071 items, painstakingly collected, not with miserly selfishness but with dutiful responsibility. Those of us who have used the collection know the graciousness of Professor Hollander in sharing the actual books; those of us who have received copies of the splendid check list of his library

realize the trusteeship which Professor Hollander has assumed for himself; those who will consult the *Economic Library* in reference rooms will find their path made easier through his help.

Fifty years ago, no systematic collections of historical economics existed. Today we have at least four: Foxwell's first collection (now the Goldsmiths Library), Seligman's collection (now in the Columbia University Library), Foxwell's second collection (now the Kress Library at the Harvard Graduate School of Business Administration), and Hollander's. I have used all four, and I feel duty-bound to express publicly my gratitude to the men who brought these collections together. The tasks which these three collectors set for themselves were not identical; as a result the collections complement each other. Among other things, for example, Professor Hollander has striven to reassemble as much of the famous Massie collection as possible. Finally, he has given us this magnificent printed record of his faithful work which will serve us as a most useful tool and will remind us that we also have responsibilities to those who follow us.

E. A. J. Johnson

New York University

Some Theoretical Problems Suggested by the Movements of Interest Rates, Bond Yields and Stock Prices in the United States since 1856, by Frederick R. Macaulay, with a preface by Wesley C. Mitchell. New York: National Bureau of Economic Research. Publication No. 33. 1938. 500 pp., 33 tables, 32 charts. $5.00.

This volume is much more than a book on interest rates and security prices, as suggested by the abbreviated title on the cover. The full title gives a much better indication of its contents, but even that does not lead the reader to expect all that he finds. The sections of the philosophical introductory chapter that deal with the problems and difficulties faced by scientific investigation in the field of economics, for instance, are applicable, not only to the problem of long-term interest rates and their relationship to short-term interest rates and to other economic phenomena but also to almost any line of investigation in the general field of economics.

This chapter has now appeared in reprint form and should be read with profit by economists and statisticians generally, even though they may not be primarily concerned with the subject of interest rates. The prevalence of attempts to forecast one thing or another, on the basis of evidences of empirical relationships between two or more statistical series, more than justifies the caution given by the author that acceptance of such evidences as though they were explanations may easily become a delusion and a snare. As Dr. Macaulay also says in his introduction, "if no explanation of why a fundamental relationship should exist can be supplied by other than a grossly *ad hoc* hypothesis, the 'generalization' may 'work' for years and then fail forever." Later on, in a digression suggested by the high coefficients of correlations that have been derived by others from comparisons of interest

rates and commodity prices, he discusses at some length the need for careful examination of the relationships between the series that produce such coefficients before concluding that there is convincing evidence of a direct causal connection between them.

In other respects the introductory chapter provides a setting for the treatment of problems that are considered in the subsequent chapters, as for instance, the need for careful definition of the problem before attempting to solve it. Before discussing even the measurement of long-term interest rates, Dr. Macaulay defines the significance of such rates and their theoretical relationship with short-term interest rates. The yield of a bond is defined as a single rate, such that the present value of all future payments of interest and principal, when calculated by assuming this rate, would equal the price paid for the bond. The yield theoretically should be an average of the various short-term rates of interest in successive future periods during the life of the bond. Such rates, being in the future, cannot of course be known but are implicit, and the implicit rates rarely correspond at all closely to the actual rates. The implied forecasts of future interest rates that are indicated by the pricing of serial bonds are frequently quite different for securities of the same maturities offered by different borrowers at approximately the same time. It is evident, therefore, that the pricing of the bonds is based on rough and ready guesses as to the yields that will sell the bonds, rather than on any definite set of forecasts of future interest rates.

In the pricing of any bond a second type of forecast is also involved—a forecast of the degree of certainty of the future payments. The realized yield may be quite different from the promised yield, and there can never be absolute security of future payments. The concept of "pure" or "riskless" interest, therefore, is metaphysical.

A new concept introduced by Dr. Macaulay in the course of his discussion of bond yields is that of "duration," which takes into account the rate of interest paid on a bond as well as its maturity. Of two bonds having the same yield and the same maturity, the bond with the higher coupon rate has the shorter "duration," as a larger proportion of the total payments will have been made at any given time prior to maturity than on the bond with the lower coupon rate.

In constructing his index of bond yields over the period from 1857 to 1936, Dr. Macaulay used railroad bond prices because sufficient quotations for any other homogeneous group of high-grade bonds were not available and even with them encountered many difficulties in eliminating the influence of factors other than changes in long-term interest rates. A chain method was necessary, since a changing list had to be used to cover so long a period; furthermore, bonds could be included for only a part of their life, as their yields were affected by their nearness to maturity. Even with that procedure, it was found that quotations for individual bonds were affected by economic "drift," which involved actual, if not nominal, changes in their grades. An ingenious method was devised to eliminate this "drift" in the construction of the index. Economic "drift," when segregated from the movements of

long-term interest rates, as such, was found to correspond much more closely with the movements of common stock prices than did the movements of long-term interest rates.

Common stock prices reflect the market's estimate of future earnings and dividends, and lower grade bonds partake of the nature of both the higher grade bonds and common stocks. Dr. Macaulay takes the position, however, that a cyclical decline in stock prices is primarily a result of pressure rather than of a change in anticipated earnings, important as such a change may be, owing to the fact that stocks, to a much greater extent than bonds, are carried on loans—a position which may be open to question, even though, after a slump in the market sets in, credit liquidation is unquestionably an important factor.

An even more questionable conclusion is that the cyclical movements of high-grade bonds and of stocks have essentially the same relation to the business cycle, since both are determined by the attendant cycle of confidence, especially on the down-swing of the cycle, because the demand then is not primarily for more secure investments but rather for more cash. But a tendency for cyclical rises in bond-yield indexes to precede, rather than to follow, cyclical declines in business indexes, is demonstrated in the last chapter, and it is not difficult to find instances of an *increased* rather than a *reduced* demand for high-grade bonds, and a decline in their yields, after a downturn in business has started. May not the cyclical movements in bond yields, except in periods of extreme depression, be related more indirectly to cyclical fluctuations in business than is explained by the cycle of confidence—through the effects of expanding or contracting business volume and changes in prices on the "money market" situation, and thus on short-term interest rates, which in turn affect long-term rates? A detailed analysis and discussion of the *actual* as distinguished from the *theoretical* relationships between long-term and short-term interest rates is not presented in this volume. Perhaps it may be expected in the subsequent volume, referred to in a footnote in the introduction, which is to contain "the tables and charts concerned with the relations of the rates and yields to credit and banking conditions, with the theoretical and statistical discussion of those aspects of the subject."

In Chapter VI the author raises serious questions concerning the presumably well-established correlation between commodity prices and interest rates. He finds that high coefficients of correlation can be obtained only by selecting certain periods and suggests that the lack of correlation in other periods is so glaring as to demand explanation. He examines the various theories as to reasons for the assumed relationship and finds none of them convincing. He presents an alternative hypothesis which, however, he admits "is almost as difficult to reconcile with parts of the actual rates and prices as are some of the theories upon which we have so adversely commented."

The final chapter presents comparisons between a number of series of data on interest rates, commodity prices, and business volume, with trends

eliminated to facilitate comparisons of cyclical movements. The procedure followed in trend elimination, however, is such that it is not too clear just what is the significance either of the trends or of the cyclical fluctuations obtained by computing deviations from the trend lines.

From the comparisons of cyclical movements thus computed, certain average leads and lags between the various series are found. The reader is cautioned that any degree of confidence in the accuracy of any averages is entirely unwarranted—that they may well be more misleading than enlightening—but the author concludes that the study of the relative timing of different economic series is at least more promising than the attempt to develop rigid cyclical formulas.

The volume ends as it starts, in a philosophical vein, with a discussion of the need, not only for a growth of knowledge and with it greater accuracy of forecasting, but also for greater effort to do those things in the present that will reduce the violence of economic disturbances in the future. "We must make the future and not merely forecast it."

More than half of the book is taken up by tables and charts, but the space devoted to the discussion is extraordinarily meaty. The volume is obviously the result of an immense amount of work and careful thinking over an extended period and merits intensive study rather than a mere cursory reading.

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Prosperity and Depression, A Theoretical Analysis of Cyclical Movements, by Gottfried von Haberler. New York: Columbia University Press. Geneva: League of Nations. Economic Intelligence Service. 1937. xv, 363 pp. $2.00.

In constructing theories of the Trade Cycle, as in other connections, economists often tend to exaggerate the importance of the particular elements around which their respective theories are built, so that the analysis of a particular force operating on the business situation is presented as a complete theory of the business cycle. In the present volume Professor Haberler has rendered a useful service in a very skillful perpetration of the opposite error.

The book consists of two parts. The first part contains an extremely lucid account of the outstanding theories of the trade cycle and a masterly extraction, in the face of divergent terminologies, of points that are common to more than one theory. (It would have been interesting to know who was the author who declared his views to be unrecognizable in Professor Haberler's summary.) In this task Prof. Haberler displays much admirable ingenuity and is able to make a large number of valuable, if minor, clarifications, e.g., in making explicit the psychological implication of the "acceleration principle" of derived demand.

The second part of the book consists of an eclectic theory of the trade cycle where Professor Haberler's genius for finding everybody right surpasses itself in discovering for every theory some section of the course of the cycle to which it is applicable. One sometimes gets the impression of a host of plausible or conceivable influences on business activity being arranged by the author in an order that generates a plausible picture of the trade cycle; but to describe Professor Haberler's synthesis as being just that would be to make a vicious caricature. The division of the cycle into the four elements of expansion, contraction, down-turn, and up-turn will be a valuable device even for more unitary theories of the trade cycle.

The analysis suffers considerably from a weakness in terminology that is not unrelated to Professor Haberler's almost pious attachment to tradition and overcharitable reluctance to find anybody quite wrong. He rejects the peculiar definition of saving used by Mr. Keynes in his *Treatise on Money* ("normal" income earned in the period—excluding profits—*minus* consumption) according to which an excess of investment over saving is synonymous with the existence of profits. Neither does he accept the "common-sense" definition used by Mr. Keynes in his *General Theory of Employment, Interest and Money* (total income earned in the period—including profits—*minus* consumption), according to which saving (for the whole economy) cannot diverge from investment. He prefers to follow Mr. Robertson's alternative "common-sense" definition (income *available* in the current period—which was earned in the previous period—*minus* consumption in the current period) according to which there can arise divergences between saving and investment. Professor Haberler appears to do this in order to be able to consider the effects of these divergences—"excess saving" and "excess investment"—on the business situation.

Quite apart from all the unanswered and perhaps unanswerable questions begged by the notion of the "period" and its different length for different purposes, this procedure is unsatisfactory because it obscures the fact that on this definition an (algebraic) excess of investment over saving is *synonymous* with an increase in money income and with "inflation."

An attempt is made to save this situation by constructing a neoclassical theory of the rate of interest with saving as one of the sources of the supply of investable funds, so that an increase of saving, by lowering the rate of interest, may increase investment and there is restored the significant question of the extent to which it will have this effect. But here we get lost in a maze of time lags of different length where almost anything can happen. The effect of the increase in saving on the *demand* for "investable funds" (which of course need not mean a desire to invest them) is lost sight of, and the criteria of plausibility that are afforded by a modern liquidity theory of the rate of interest are absent. In this confusion a theoretical place is found for the "under-saving" theory of the breakdown of the boom at the cost of laying aside the obviously net deflationary effect of a decline in the demand for consumption goods (which is recognized on p. 216) and an abdication from any specific pronouncement on the general effect of public works.

It would also appear to be unsatisfactory to pair people's attitudes to their *flow* of saving and their *stock* of money as joint and parallel influences on the supply of loanable funds in the determination of the rate of interest, and a concentration throughout on MV certainly pays a disproportionate respect to the quantity theory of money and to the independence of MV and is liable to allow the student to slip very easily from MV or "the effective amount of money" to M. The inadequate distinction between stocks and flows and between levels of employment and rates of change in this level (which leads Professor Haberler himself to remarkably few mistakes) is also responsible for the unsatisfactory definitions of prosperity and depression (p. 162) that do not make it clear which we have when employment is high but falling or low but rising.

All of these and other weaknesses seem to spring from the same source—a failure to adopt explicitly the modern formulation of the theory of the rate of interest in terms of the supply of money and liquidity preferences, and to make the sharp distinction between saving and time preference on the one hand which deals with flows; and cash and liquidity preference on the other hand which deals with stocks. Should Professor Haberler overcome his dislike for this method and free himself from the incubus of the cumbersome traditional terminology, he will undoubtedly be able to use his skill to much better purpose and for more positive contributions on the lines indicated by the work of Harrod and Kalecki.

With all these weaknesses in the synthesis, the book remains a valuable handbook of the existing theories of the trade cycle and a salutary reminder of the complexity of the problem to those who tend to oversimplify.

A. P. LERNER

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Studies in the Social Aspects of the Depression: Crime in the Depression by Thorsten Sellin, *Education in the Depression* by The Educational Policies Commission, *The Family in the Depression* by Samuel A. Stouffer and Paul F. Lazarsfeld, *Internal Migration in the Depression* by Warren S. Thompson, *Minority Peoples in the Depression* by Donald Young, *Recreation in the Depression* by Jesse F. Steiner, *Religion in the Depression* by Samuel C. Kincheloe, *Rural Life in the Depression* by Dwight Sanderson, *Social Aspects of Consumption in the Depression* by Roland S. Vaile, *Social Aspects of Health in the Depression* by Selwyn D. Collins and Clark Tibbitts, *Social Aspects of Reading in the Depression* by Douglas Waples, *Social Aspects of Relief Policies in the Depression* by R. Clyde White and Mary K. White, and *Social Work in the Depression* by F. Stuart Chapin and Stuart A. Queen. New York: Social Science Research Council. 1937. Bulletins 27–39, $1.00 each or $10.00 a set.

The social aspects of the great depression of the early 1930's cannot be adequately evaluated at present, for the various phenomena observed during

these years must be viewed against a historical perspective. When this perspective is attained, not the least important aspect may well prove to be the effect of the depression upon the sociologists themselves, as evidenced by the research approach developed in the thirteen monographs sponsored by the Committee on Studies in Social Aspects of the Depression of the Social Science Research Council.

The concept of *trends* has long been firmly entrenched in sociological theory. Social trends have been studied as important in and of themselves, but little interest has developed—up to the publication of these monographs —in the magnitude or characteristics of the recurring positive and negative deviations from these trends.

Perhaps the most important aspect of the research approach developed in these monographs is the introduction of the concept of *flux and reflux* as applied to social norms, and the recognition of the complex interrelationships between social trends and social cycles, as indicated, for example by the following quotation from the monograph on *Religion:*

> Religion makes its basic changes slowly. The big question to determine is that of the degree to which the changes occurring during a time of depression were already under way and were simply speeded up or retarded by the depression, and to what extent the depression created situations so different that new factors began their work. In many cases the old trends probably continued, but were influenced by conditions due to the depression (p. vii);

or the following from the monograph on *The Family*, whose authors point out that, whereas they are dealing with a primary institution and sociological theory postulates "that the deeper the structure of an institution is rooted in the mores the more resistant is it to change," nevertheless,

> the decline in the marriage rate [during a depression] has direct demographic effects in the reduction of births, sociological effects in possibly encouraging a relaxation of sex taboos among the unmarried, and doubtless psychological effects on individuals, particularly on the women who fail to marry. Thus the family, however much or little it may alter as an institution, may play an important role as a modifying factor during depressions (p. 7);

or the statement of the author of *Minority Peoples*, who denies to the depression "an originally creative role" but points out that this denial "is not to be construed as a refusal to recognize fundamental shifts in emphasis and even reversals of trends in minority life when these are really consequences of the depression" (p. 16).

The authors were expected to define significant problems in their fields, from the point of view of possible depression effects; to evaluate the state of existing knowledge; and to point out promising and feasible research problems to further the development of this knowledge. The success of the monographs, however, lies in the structure built for future research, the introduction of business cycles as factors to be taken into account in investigations of social processes, social behavior, and social institutions, and as contributions to method; but not so much in the definition and identification of sig-

nificant depression problems, *per se*. Their relative failure in this last respect is understandable: no frame of reference existed up to the publication of this series, and, furthermore, the working hypothesis developed by the Committee that "the repercussions [of the depression of the early 1930's] were so far reaching that scarcely any type of human activity was untouched" (Foreword of all monographs, p.v) was not such as to lead to delimitation.

Whenever existing data warranted it, the historical approach was recommended; whenever these data were quantitative, this approach followed the time series type of analysis developed by the economic statisticians. This latter was feasible only in three of the thirteen fields considered, viz. the family, crime, and health. Even in these three fields, however, limitations of accessible data render analyses of other than gross relationships extremely difficult. Regarding the family, for example, Stouffer and Lazarsfeld accept as "rather well established facts" that during a depression "fewer families are begun, by marriage, . . . fewer families are augmented by births . . . [and] fewer families are broken by divorce" (p. 5) but point out that "the structure of 'the family' in the United States is different in different groups" and state or imply that adequate time series analyses of these factors would involve subclassifications by at least age, employment status, relief status, residence in rural or urban areas, by regions and perhaps "plane of living," by race, nativity or color (and, if possible, more adequate criteria of "minority groups") etc. Regarding crime, Sellin points out that many indexes of crime have been available in time series but that a major difficulty in analyzing the relationship between crime indexes and economic indexes is the temporal shift in the unit of recording:

> The student of birth rates knows that births may not be recorded as fully during a certain period as compared with another, but he at least knows that what he does count remains unchanged in character. The unit of counting is constant. The student of crime rates, on the other hand, does not only face the need for determining the effect of changing public attitudes and administrative practices on the recording of crime data, but he finds that the definitions of specific offenses are more or less elastic, so that for one year he may have to count as larceny, for instance, some form of conduct which could not have occurred or which was not punishable in earlier years or which was labeled by some other name and therefore counted with the units of an entirely different category. The constancy of the units of offenses counted is therefore at present illusory (pp. 11–12).

His analysis further makes clear that the student of crime must also be in a position to differentiate among the various indexes of crime which are available; he must find an index which is sensitive, which is "based on offenses that are highly 'reportable' or 'detectable' " (p. 69); which is preferably for specific types of offenses; which is subclassified regionally and by social classes of the population.

In regard to measures of health, various indexes are available: "mortality, sickness, and the prevalence of physical defects including malnutrition and

underweight" (p. 11) but Collins and Tibbitts point out that mortality data, while not the most precise measure of health, provide almost the only reasonably satisfactory historical time series. These authors also indicate the necessity of specificity in the series. For example, the positive correlation between death rates and business cycles, which has been observed by a number of investigators, cannot be interpreted adequately until time series are found for

> specific groups, such as deaths from specific causes at specific ages. The breakdown that suggests itself as most pertinent would be deaths from certain causes for males of working ages, say 20–64 years. While it would be desirable to have rates that were also specific for type of occupation, at least to the extent of a separate rate for the laboring classes, no accurate data are available for such statistics. Because of aging of the population, adjustment for age differences probably should be made unless the age groups include a span of not more than 20 years. It would be important to compile the statistics for certain states and large cities separately, and compare the trends in agricultural states with those in industrial states (p. 27).

Further problems in the use of time series of vital statistics, pointed out by these authors, are that deaths have not been consistently allocated to residence and that intercensal and postcensal population bases, on which these rates are computed, are subject to cyclical fluctuations which cannot, at present, be accurately estimated. The former difficulty is, of course, shared with the student of the family in his time-series of births, marriages and divorces; the latter waits for its solution upon the student of migration.

In so far as the problem of social aspects of the depression depends on historical time series analysis, then, the authors of these three monographs are in essential agreement: Little more than gross relationships can be determined with data now available for a span of years and covering an adequate number of cycles. The problem is to lay immediate plans for future time series. The essential technical points in respect to these series are: (1) constancy of the unit of measurement, (2) sensitivity of the index chosen, (3) significant subclassification, and (4) accuracy of the population base. The third of these points is made explicit in regard to another field of investigation (*Minority Peoples*) by Young, who underlines the necessity of "valid subgroup definitions" (p. 5) in reaching true generalizations about the disparities of minority population groups. Census subclassification, he points out, is oversimplified for research purposes since only foreign-born whites and their children are classified by national origins. Thus "groups characterized by distinct alien cultures and related problems of intergroup relations" (p. 4) are lost to research. Crossnational subclassification (e.g. Jews) and subnational (e.g. North Italians and South Italians) are equally important in sociological studies. Valid subclassifications of Negroes by region of origin, education, and economic status; of Indians by "tribal" and "assimilated groups," and of Japanese by "castes" are among the most elementary essentials for determining differentials.

The authors of all the monographs recognize that even with favorable future developments in data collection the continuous time series technique will be applicable to only a few aspects of their problems. They point out that a whole cluster of problems can be approached with data in the form of discontinuous time series, however, and that this approach can be so organized as to include both qualitative and quantitative items. The recommended procedure is that of repeated surveys, or censuses. Stouffer and Lazarsfeld, for example, point out the possibility of studying marital separations from census schedules. Separation would be defined in terms of spouses resident in different communities or at different addresses.

> With all its defects, the statistical concept of separation presents definite research possibilities for studies in areas which took a census at some point during the depression. Let us assume that the percentages separated among all married persons were known for 1910, 1920, 1930, 1935 (or earlier depression year), and 1940 in some area (p. 75).

If such data were available, the authors further assume that a trend could be determined and that the depression deviation from this trend could be estimated. But censuses are quite fortuitously timed with respect to phases of business cycles and the accurate determination of a trend on this basis would introduce greater difficulties than they admit. According to Thorp's business annals, 1910 was a year of recession, 1920 (at the time of the census) was in the prosperous phase, and 1930 was a year of depression. Thus the probability of throwing light on recent depression phenomena with such a series is very slight. The repeated field survey affords a more elastic approach and is applicable to a wider range of problems, for the timing can be determined in accordance with comparable phases of business cycles. Even this approach, however, may introduce extraordinary difficulties, due to the unknown lengths of the time lag in the response of social phenomena to business cycles.

The difficulties suggested by this approach become minimized, however, in comparison with the difficulties faced by many of the authors in attempting to evaluate an event or combination of events unique to a single depression. The recent situation in agriculture is a case in point: the beginning of the agricultural depression antedated the industrial by almost 10 years; two unprecedented droughts were coincident with the industrial depression; and the federal government established a new series of agencies to assist agriculture. To disentangle the "social aspects of the depression" from this network of factors is a Herculean task. Sanderson, in the monograph on *Rural Life* handles the problem with skill and insight. Another similarly competent performance is the Whites' evaluation of *Relief Policies*. Certain of the other authors met with less success, particularly when dealing with a field where a phenomenon was first observed in the 1930's. In such cases, the allocation of "nonexistence" to a specific pre-depression year may be especially misleading. For example, the monograph on *Consumption* contains statements of which the following is typical: "auto radios have increased from none in 1930 to 3 million in 1936" (p. 24).

This series of monographs should be further evaluated as contributions to method. Stouffer and Lazarsfeld, on the one hand, and Donald Young, on the other, have made especially significant contributions from this point of view: the former in respect to their statement of the logical interdependence of the case and statistical methods; the latter, in his development of a methodology for approaching problems of cultural disparity.

DOROTHY SWAINE THOMAS

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Statistical Methods Applied to Economics and Business (Revised), by Frederick C. Mills. New York: Henry Holt and Company. 1938. American Business Series, General Editor, Roswell C. McCrea. xix, 746 pp. $3.75.

Fourteen years have elapsed since the first edition of Professor Mills' book appeared, and during those years it has proved to be one of the best American texts available. The author states in the preface to the revised edition that "account has been taken of the more important of the recent developments that have a bearing on the economic and business applications of statistics. In doing this I have sought to retain the main features of the first edition. A systematic development of the fundamentals of statistical method is needed by the beginning student. A working compendium of procedures, with necessary aids to calculation and reference tables, is required by the statistician engaged in administration or research. The book is designed to meet these two needs."

There are two types of textbooks, one which requires much supplementary explanation and interpretation on the part of the instructor and one which by its clearness of style, organization of material, and completeness of detail represents an intellectual entirety which results in a lasting companionship between text and student. Professor Mills' book belongs to the latter type.

Without reference to the historical development of statistical methods, the author proceeds to introduce the subject matter at once. In conformity with the purpose of the text, as outlined in the preface, it was doubtless felt that introductory chapters on "Statistical Methods and the Problems of Economics and Business" to be followed by chapters dealing with "Graphic Presentation" and the "Organization of Statistical Data," respectively, assure immediate pedagogic results. Not mentioned in the references following the chapter on Graphic Presentation is the important contribution by H. Gray Funkhouser, which appeared in *Osiris*, Bruges (Belgium), Vol. III, Part I, 1937, pp. 269–404 under the title "Historical Development of the Graphical Representation of Statistical Data."[1] Interesting comments on the significance of Funkhouser's historical study by J. M. Keynes will be found in the June, 1938, issue of the *Economic Journal*, London.

With minor changes in the text, the basic content of the first thirteen

[1] EDITOR'S NOTE: See page 623 of this issue for a review of this contribution.

chapters has remained essentially the same. The changes consist on the whole of the revision of practically all charts and tables. In tradition with the first edition, tables are frequently accompanied by details of computations, an excellent technique and of great aid to the student. The summarization at the end of each chapter and the appended references are not less valuable.

The text of chapter XIV, "Statistical Induction and the Problem of Sampling," has been completely revised. Discussed here is the technique of sampling in the light of more recent developments.

The introduction to the analysis of variance in the new chapter XV of the revised edition is likely to be less readily absorbed by the student unless accompanied by discussions on fundamental concepts in the classroom. It should be remembered that at this stage of the text the student hears for the first time such terms as "degrees of freedom" and "coefficient z," in connection with Fisher's theory of variance. He doubtless will be confused by the unceremonial introduction of these important strangers and will want to know a little more about their origin and their relationship to the older and more familiar members of the family. Fisher's theory of variance, while not accepted by all schools of methodologists as *lex terrae*, is, to say the least, subtle, both in application and in interpretation. It will appear even more so to the student who first comes in contact with it. The situation encountered here is comparable with that experienced when the subject of differential calculus is introduced to students who have been accustomed to the less sophisticated modes of algebraic operations.

It has been somewhat of a puzzle to the reviewer why, on the whole, authors of statistical texts have seen no reason to include in their texts an exposition of the theory of attributes and its expansion, similar to the first chapters of Yule and Kendall's *An Introduction to the Theory of Statistics*. This approach would furnish the student not only with an excellent discipline in logic but, most important of all, would pave the way to a better understanding of statistical induction, of the problem of sampling, and finally of the analysis of variance. Moreover, the theory of attributes and the exposition of consistence of data are concepts which, once absorbed, will guard against the commission of elementary statistical fallacies.

Chapters XVI and XVII, "The Measurement of Relationship: Multiple and Partial Correlation" and "The Measurement of Relationship and the Problem of Estimation," respectively, have remained the same in their basic structure.

Chapter XVIII, "Statistical Induction and the Problem of Sampling, Concluded" is new and includes sections which deal with small samples, the transformation of r, and the nature of Chi-Square and its distribution.

To the revised appendix, which consists of five sections, much new material has been added. Section D contains detailed instructions for computing a modified exponential curve, a Gompertz curve, and a logistic curve, all of which are valuable for reference purposes. Section E, entitled "A Further Application of Variance Analysis," is an extension of chapter XV to illustrate

by additional examples the application of Fisher's method of variance analysis.

Appendix F, "Glossary of Symbols," and ten appendix tables, five of which are excerpts from tables appearing in R. A. Fisher's *Statistical Methods for Research Workers*, a list of references, and an index conclude the book.

The new edition of Dr. Mills' book will continue to be of valuable assistance to the student who is in need of a practical reference text and of a technical introduction to the fundamental operations of statistical methods.

R. von Huhn

Washington, D. C.

A First Course in Statistics, by E. F. Lindquist. Boston: Houghton Mifflin Company. 1938. xiv, 226 pp. $2.25.

Study Manual for A First Course in Statistics, by E. F. Lindquist. Boston: Houghton Mifflin Company. 1938. vi, 122 pp. 80 cents.

Following the current trend toward making introductory courses in educational statistics as easy as possible for those students who are not mathematically trained, the author has prepared a textbook which should accomplish this purpose to a remarkable degree. It has been designed to cover a one-semester course at undergraduate or graduate level.

An idea of the topics which are included may be gained from a list of the chapter headings, namely, "The Frequency Distribution," "Percentiles," "Graphical Representation of Frequency Distributions," "Measures of Central Tendency," "Measures of Variability," "The Normal Curve of Distribution," "Sampling Error Theory," "Standard Measures and Methods of Combining Test Scores," "Correlation Theory," and "Correlation Techniques Applied in the Evaluation of Test Materials."

The book is written in a facile style, much like a series of informal lectures. Its central theme is the use and interpretation of the various statistics, with only minor emphasis upon formulae and methods of computation. The *Study Manual* which accompanies the text should be considered a part of it. The reader is warned in a number of places that the discussion in the text is incomplete and that the questions in the *Study Manual* must be considered in order that an adequate understanding may be attained. The *Manual* is particularly valuable in this respect in connection with the chapters dealing with percentiles, graphical representation, measures of central tendency, and measures of variability. In the words of the author. "Much of what has formerly been *presented to* [the student] (for memorization) is here *drawn out* of him through leading questions and suggestive illustrations."

There are a number of minor points of criticism which might be worthy of mention. For example, the reviewer would not agree with the author that an interval of five units for a frequency distribution should have its midpoint fall on a multiple of five; on the contrary, it is far easier to tally data if the lower integral limits (as distinguished from the real limits) are always multiples of the size of the interval.

Having bent all his efforts toward making his subject comprehensible to the nonmathematical student, the author has perhaps neglected those students who might prefer to think in mathematical terms. Four pages are devoted to verbal instructions for computing percentiles and percentile ranks. Nowhere, however, does there appear a formula for either of these measures. Likewise, there are four pages of instructions for computing a mean from a frequency distribution, again with no formula.

Possibly the topic which has been least adequately treated is that of significant figures. The discussion, although lengthy, refers specifically to the number of significant digits which should be retained in the mean and fails to generalize to other situations. No instruction is given relative to the rounding of numbers. Although most of the numbers in the numerical examples throughout the book are properly rounded, some are not. On page 76 the square root of 3.998 is given as 1.99, which should have been 1.999 or 2.00, and which leads to an error of 0.05 in subsequent calculation.

The following typographical errors have been noted: page 101, the reference in line two should be "page 84"; page 120, formula (17), the σ_2 to the left of the equality sign should be written as part of the subscript; page 131, the end of the second paragraph should read "Table 19."

The value of Dr. Lindquist's contribution lies in his clear presentation of the various statistical concepts, both in the *Manual*, as noted above, and in the text, particularly the normal curve and correlation theory. The text could hardly serve as a reference book and should not be employed without the *Manual*. The latter, on the other hand, could be used to advantage with any textbook in educational statistics.

FRANCES SWINEFORD

The University of Chicago

Tests of Normality, By R. C. Geary and E. S. Pearson. England: Issued by the *Biometrika* Office, University College, London, and printed at the University Press, Cambridge. 1938. 15 pp. One shilling.

Although for large samples the β_1 and β_2 tests of the normality of distributions have long been among the tools of statisticians, it is only during the past ten years that progress has been made in deriving the exact sampling distributions of the usual estimates b_1 and b_2 of β_1 and β_2, respectively. In these few pages, which are, perhaps, best regarded as a separately issued chapter of an elementary textbook on statistical methods, the authors present results obtained in previously published research on the exact sampling distributions of b_1, b_2, and a, where a is the average deviation divided by the standard deviation. The statistic a has been introduced as a possible substitute for b_2, since the sampling distribution of the latter is too skew for easy use and since the fact that a and b_2 have a high negative correlation makes it possible to consider a to be a measure of kurtosis. Examples dealing with grouped and ungrouped data are worked out in detail. Computing charts which greatly reduce the necessary work are included.

The monograph is clearly and nonmathematically written. References are given to the theoretical research on which it is based. It makes available, for routine computation, statistical methods which, in that they are easy to use and are valid for samples of any size, are superior to the techniques now commonly employed for the same purpose.

WILLIAM G. MADOW

Milbank Management Corporation
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Historical Development of the Graphical Representation of Statistical Data, by H. Gray Funkhouser. Bruges, Belgium: Saint Catherine Press Ltd. *Osiris*, Studies on the History and Philosophy of Science, and on the History of Learning and Culture, Edited by George Sarton. Volume III, Part I. 1937. pp. 269–404.

The story of how graphic methods came into being should prove interesting reading to all statisticians. Dr. Funkhouser's recent work represents the first serious attempt to trace the development of the graphic method from its early beginnings. No doubt to most readers it will be a surprise to learn that excellent charts were being made over a century and a half ago.

Dr. Funkhouser has dug deep into the records of the past to find examples of graphic devices used to present statistical information. He traces the elements of the modern chart back to very early times. The principle of coordinates he places in Egypt and Greece long before the time of Christ. He has discovered a representation resembling a modern chart in a tenth century Latin treatise on astronomy. The concept of presenting theoretical data graphically seems to have been developed by the French mathematician Oresme in the fourteenth century, although the first example located by the author of the presentation of observed data in graphic form was a drawing of the variations of the compass prepared in 1701 by the English scientist Halley. The first published instance of the practice of representing time as a line appears in a treatise prepared in the middle of the eighteenth century by Joseph Priestly, the English chemist. Dr. Funkhouser refers to the contributions of many other scientists during this early period, but it is to William Playfair that he gives most of the credit for originating charts such as we know today. Certainly the charts included in the treatise, some of them originally published as early as 1786, are surprisingly modern in their general appearance and conform in most fundamental respects to the best practice today.

Some of the most interesting material contained in Dr. Funkhouser's work has to do with the history of attempts to standardize graphic methods. In connection with the meetings of the International Statistical Congresses held in Europe between the years 1853 and 1876, the graphic method became the subject of much discussion, particularly as regards the possibility of making it an international language in the scientific field. Dr. Funkhouser

devotes considerable space to the arguments for and against standardization voiced during this period, but unfortunately he does not carry the story through to the present time. The standardization movement in the United States during recent years he dismisses with a very few words. He does mention the Joint Committee on Standards for Graphic Presentation, formed under the sponsorship of the American Society of Mechanical Engineers in 1913, and refers to the preliminary report the Committee published in 1914, but then he makes the statement that "nothing further than the issuance of the foregoing preliminary report seems to have been done by this Joint Committee." As a matter of fact, this committee worked for several years after this on the compilation of recommendations as to graphic principles and procedures and the committee was, in effect, revived in 1926 when the American Society of Mechanical Engineers formed a joint committee of representatives of national societies, known as the Sectional Committee on Standards for Graphic Presentation, a committee that is still active.

The part of the history having to do with the application of the graphic method during the past 30 or 40 years seems quite inadequate. Apparently no serious attempt was made to cover all the fields in which the method has come to be used, and no mention is made of the multitude of charts and diagrams being prepared for purposes other than publication, for example, charts prepared for administrative use in the business and industrial fields where, during the past twenty years, the graphic method has become a generally accepted tool of management. Dr. Funkhouser probably should not be criticized too severely, however, for the omission of unpublished material, for any thorough-going survey of all applications of the method would be a sizable undertaking indeed.

The reference value of the treatise has been considerably enhanced by the inclusion of an excellent annotated bibliography of some twenty pages, listing American and foreign publications relating to the subject of graphic presentation or including examples of the method. On the whole, statisticians should be grateful to Dr. Funkhouser for the material he has assembled, particularly that culled from early sources which provides a wealth of evidence that the graphic method has had a worthy ancestry.

A. H. RICHARDSON

New York, N. Y.

Kapitalexport und Schuldentransfer im Kontjunkturverlauf, by Dr. Peterheinz Werhahn. Jena, Germany: Verlag von Gustav Fischer. Probleme der Weltwirtschaft. Schriften des Instituts für Weltwirtschaft an der Universität Kiel. Herausgegeben von Prof. Dr. Andreas Predöhl, 60. 1937. viii, 186 pp. RM 9.—

This book offers a survey and an analysis of the statistics of the balance of payments of many countries during the postwar period and especially in the depression years. It departs in two directions from the traditional ap-

proach. By theoretical reasoning and factual analysis it substantiates arguments which have been proposed as a criticism of orthodox theories on the one hand and of a merely statistical interpretation on the other hand.

The orthodox theory of international capital transactions described the mechanism by which a transfer of money from one country to another country leads eventually to a corresponding movement of merchandises by which the monetary transfer becomes a real one. Werhahn distinguishes the transfer of capital from the transfer of interest. The transfer of capital usually will not meet difficulties. The fact that borrowers succeed in meeting capital demand often creates conditions through which the transfer can be realized. There will be usually a demand for capital in a country only when the general business conditions are favorable, and then there probably will be little difficulty in executing the transfer.

The case of a transfer of interest payments from a debtor country to a creditor country in different. As long as these payments are compensated by capital transactions in the other direction the difficulties often will not become apparent. When in a depression, however, the capital movement stops, a real problem may arise, and it will be aggravated when the prices are declining while the nominal amounts of the interest charges remain the same. A transfer in gold will not induce the central bank in the creditor country to expand the volume of credit and to raise the price level (as it should be expected, according to the orthodox theory). The attempt of the debtor country to enforce exports will not meet an expanding demand in the creditor country. The producers of the latter country will request a protective trade policy against the competitor. The debtor country then will resort to a restriction of imports in order to establish the export surplus which is necessary for the transfer of the payments. This restriction will result in a further contraction of purchasing power in other countries. Moratoria for international debt services will result, often only after the attempt to enforce the transfer has contributed to an aggravation of the depression. This picture certainly is quite different from the one sketched by John Stuart Mill and his successors. Recent experience has proven that under certain conditions and in certain phases of the business cycle such a pessimistic theory is more realistic than the orthodox theory.

For a merely statistical interpretation of the balance of payments the various amounts are nothing but items in an equation. A merely statistical analysis cannot establish any causal relationship among various items. The author applies a classification of the various items according to the *autonomous* changes which result from events outside of the balance of payments and the *reactive* changes by which a change in other items is compensated. If, for instance, an increase in imports is financed by short-term borrowing, this capital import is regarded as a reactive change compensating the autonomous change which occurred in the imports of the country. This analysis leads to a dynamic interpretation of the balance of payments.

In the statistical interpretation of the balance of payments mostly net figures of the balance of capital and interest payments are compared with

the net balance of trade and services. The author finds cases in which, for instance, an import of capital and a payment of interest compensate each other so that no influence is exerted upon the trade (for instance in the post-war conditions of the South American debtor countries). In other cases he finds, however, that the capital import is realized by an import of machinery and other industrial equipment, whereas the interest payments are executed by an export of raw materials. Under such conditions—the author gives the Eastern and Southern European countries as examples—the effects of capital import and interest payment do not offset each other, but each one affects the balance of trade, the one the import, the other the export side.

I report in some detail on these points of view which the author takes in his analysis, since I regard them as an improvement compared with the traditional approach. These points of view were, of course, not originated by the author, but he uses them successfully in the analysis of a rich statistical material.

GERHARD COLM

New School for Social Research

The New York Bond Market 1920–1930, by Charles Cortez Abbott. Cambridge, Massachusetts: Harvard University Press. 1937. Harvard Economic Studies, Volume LIX. xvii, 224 pp. $2.50.

In the introductory chapter, the author states that the study was initially undertaken in the statistical department of the National City Company. In its original form it was essentially an analysis of "contemporary happenings." Its primary purpose was to "throw some light on those deep-lying economic forces which determine the success or failure of new security issues." Subsequently the scope was enlarged to include an analysis of the relationships of the banking system to the capital markets in the period covered and an evaluation, through the use of statistical data, of the monetary theory then current, by which the author apparently means the doctrines set forth by Professor Keynes in his *Treatise on Money*.

The study opens with a description of the characteristics and peculiarities of the bond market. Emphasis is given its importances in the economic life of the country. The various sources of demand for bond investments as well as the changes that occur through time in the quality of bonds are analyzed. Attention is directed to the important interactions between short- and long-term rates of interest.

Following upon this general discussion of the characteristics and problems of the bond market, there is presented a complete statistical review of developments from 1920–1930. Data are included of prices, yields, the volume of trading, the volume of gross and net bond issues, etc., and estimates are presented of the year-to-year changes in the bond holdings of various classes of investors. This period is then divided into six separate segments, with a

chapter devoted to a detailed treatment of each. The author's significant conclusions and interpretations are summarized in the final chapter. Supporting statistical data are presented in appendices; and a bibliography, with reference to the pertinent works in the field, is included.

One of the conclusions reached by the author in consequence of his studies is that the volume of bond flotations is more influenced by the current price of capital than by the needs of industry for long-term funds (pp. 21–22). It must not be assumed from this statement that easy credit conditions are always the controlling factor. In 1921, the desire of business concerns to refund bank loans led to bond issues at a time when money rates were high (p. 74). In general, however, "the amount of capital sought at any given time through bond issues is ordinarily affected to a very high degree by the relative cheapness or dearness of borrowing" (p. 19). To this statement should be added the important qualification that its correctness depends upon an absence of change in entrepreneurial expectations at different points of time.

Bank buying, representing the principal demand for long-term bonds, is not in itself influenced by the long-term rate of interest. Whether commercial banks purchase bonds or not depends upon the amount of their excess reserves. Through this period excess reserves to a large extent were a function of the open-market operations of the Federal Reserve banks. The consequence was that when the Federal Reserve banks were adding to their open-market portfolios, member bank excess reserves were large and the bond market was buoyant. Bank credit rather than savings equated the demand for and the supply of capital issues.

A development, to which the author gives repeated emphasis, was the loss of relationship between short- and long-term rates of interest. Through much of the period studied, the two proceeded along different courses (pp. 145–48). In the five years from 1923–1928, movements of short-term rates had little effect upon movements of long-term rates (p. 100). A diversity of this character, Professor Abbott declares, "can be taken as evidence that the financial mechanism is subject to unusual influences" (p. 145).

In consequence of his studies, the author is not one to adhere to the oft-expressed notion that the Great Depression was the result of oversaving and of underconsumption and that the remedy lay in increasing the volume of monetary purchasing power. In his opinion, investment outran savings in the decade of the twenties (pp. 150–51).

Through the volume, pains are taken to analyze carefully the reasons for and the economic implications arising from the growth in funded debt. The effect on the banks through the alteration in the character of their assets, the effect on the public, and the effect on cost rigidities of business are discussed.

The volume may be characterized as a discussion of the bond market from the money market point of view. The analysis, as the author states himself, is in terms of short-time periods. Interpretations proceed from an analysis of the supply of and demand for reserve funds. In consequence the volume

supplements those studies which approached the subject from a more abstract standpoint and is a valuable addition to the growing literature on savings and investment.

BENJAMIN HAGGOTT BECKHART

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Financial Development of the United States, by William J. Shultz and M. R. Caine. New York: Prentice-Hall, Inc. 1937. xxviii, 757 pp. $5.00.

Financial histories all too often have been concerned mainly, if not exclusively, with government finance and with monetary and banking developments. The authors of this volume have sought to include various other phases of private finance as well in writing a financial history of this country from colonial days to the end of the first administration of Franklin D. Roosevelt.

The chief problem that arises in such an ambitious undertaking is to redefine the scope of the field. In their introduction the authors unhappily define finance as "all factors involved in transactions in values," which of course far transcends the ground they cover. Actually they seek to integrate available historical material on government finance, money and banking, the development of the capital market; and corporate combinations. The evolution of corporate accounting practices, stock exchange methods, consumer finance, and certain other phases of financial evolution are touched upon little, if at all.

A second basic problem confronting the authors was to mark off the various epochs of financial development. They chose to distinguish fully 20 separate periods from 1600 to 1936, and to treat each era distinctly. Although this procedure has enabled them to compress a vast amount of factual material into their book, it has tended to hamper broader analysis and the study of the interrelation of the major forces which shaped financial development.

The result is a readable and interesting narrative which should prove of special value to students of finance who have found the available historical literature concerned either with economic development in general or with particular phases or periods of finance.

JULES I. BOGEN

The Journal of Commerce
New York City

Studies in Income and Wealth, Volume One, by the Conference on Research in National Income and Wealth. New York: National Bureau of Economic Research. 1937. xviii, 348 pp. $2.50.

The meeting sponsored by the conference, and here reported, was a cooperative venture by several universities and economic research organizations, in an attempt to lay the conceptual foundations for later statistical

research in national income and wealth. The various controversies concerning national income which have enlivened the dinner hours in economic research bureaus for almost 20 years are here put in print for a wider audience, and the result is a contribution to economic and statistical thinking.

The report contains papers on the general question of the definition of national income, written by some of the men most interested in national income estimation, namely, M. A. Copeland, Simon Kuznets, and Clark Warburton. It proceeds from these general definitions to their accounting and taxation aspects, discussed by some of the above-named men and by specialists such as Solomon Fabricant, Gerhard Colm, Carl Shoup, Mabel Newcomer, and Boy Blough. The final parts are papers on labor income by Solomon Kuznets and on income parity for agriculture by O. C. Stine.

Most of the puzzles which have confronted statisticians in this field are mentioned, such as the imputed income of housewives and of home owners, government income and interest on government bonds, relief and subsidy payments, general price level changes, accrued versus received bases of computation, capital gains, entrepreneurial withdrawals, and international payments. These items are treated variously according to each writer's preconceptions, and some light is thrown on the nature of these preconceptions. Some agreement is reached, mainly on the thesis that there are between three and five different types of measurement of national income, but no agreement on the question whether all types would, with complete measurement, reach the same total. In particular, it is pointed out that the most common definition of national income, namely, a flow of goods and services, is almost never used in actual measurement.

That this discussion did not succeed in getting down to the fundamental conceptual difficulties is a reflection not upon the eminent participants but upon the state of modern economic thought. Still dominated as it is by the Ricardian cost concept of value, despite minor concessions to the Austrian school and a nod to the institutionalists, and still strongly materialistic, economic theory today offers little except handicap to the statistician. Accounting theory and practice, in so far as it is also dominated by the cost concept of valuation, is a further handicap. This book reveals rather than satisfies the need for some heavy thinking on the nature of income and wealth.

Ethical preconceptions, ranging from the "productivity criterion" of income to the rejection of products not considered socially acceptable, are apparent throughout the discussion, and are openly acknowledged by some of the writers. In arriving at net income, the cost of keeping a horse is always subtracted from the gross income of his services, but the minimum cost of keeping a man is never subtracted from the gross income of his services, perhaps, as Irving Fisher once said, because to do so would reveal certain classes of people to be receiving no net income! Attitudes toward the spread of government activities and toward relief and agricultural subsidy payments may or may not influence the decision whether to include such items in the national income.

Progress seems to have been made in these papers toward distinguishing between the "flow of goods and services" and the concept of "available" or "disposable" money income. The difference between these concepts, when unrecognized, has always confused discussion. If the idea that money means more than the purchase of materialistic goods and services, that it means also prestige, power, security, and other intangible elements whose ultimate translation into goods and services is remote or improbable (an idea partly suggested by Clark Warburton's paper and partly implied there), ever is accepted, the reconstruction not only of the statistical but of the economic theory of income will have begun.

Attention perhaps should be called to the paper on corporate savings by Solomon Fabricant, the algebraic treatment on intra-year price changes and their effect on inventory valuation by Simon Kuznets, and the fine illustrations of the influence of accounting methods on income totals by Clark Warburton, as special contributions of interest. In general, the book will be a stimulant to thinking for many years.

EDGAR Z. PALMER

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National Income and Capital Formation 1919–1935, by Simon Kuznets. New York: National Bureau of Economic Research. Publication Number 32. 1937. x, 86, pp. $1.50.

This volume is a preliminary summary report of two studies—one of national income and the other of capital formation—carried on during the past four years by the National Bureau of Economic Research under Dr. Kuznets' direction. Some material from another study of the National Bureau—that of capital consumption under the direction of Mr. Solomon Fabricant—has also been incorporated. Since the full report of none of the three studies has yet been published, and details regarding sources and methodology are omitted from this summary volume, no general appraisal can be made in this review of the methodology employed nor of the validity of the estimates. The review is, therefore, confined to the concepts selected by Dr. Kuznets for statistical measurement, some of the basic procedures in preparing the estimates, and a few of the most important results.

Dr. Kuznets retains the definition of national income which has long been used by national income investigators in the United States but has dropped the term "national income produced" which was used in some of his former publications to designate this concept. He states this definition as follows:

> Net national product or national income may be defined as the net value of commodities and services produced by the nation's economic system. It is "net" in that the value of output of all commodities and services is reduced by the value of commodities (fuel, raw materials, and capital equipment) consumed in the process of production.

Dr. Kuznets' measurements cover not only national income but also gross national product, which includes, in addition to the items making up the

national income, the value of durable capital goods used up—that is, the amount of depreciation and depletion—in the process of production. He points out that the value of the national product, whether gross or net, may be measured at any stage in the circulation of economic goods—production, distribution, or consumption—with results that should be identical, provided statistical data are adequate; but that judgments as to what constitutes economic activity, and judgments as to what constitutes commodities and services, income shares, consumption, savings, etc., are of telling importance in the measurement of the value of the national product.

With Dr. Kuznets' definition of national income, and with the foregoing general observations regarding national income measurements, other economists, are, so far as the reviewer is aware, in full agreement. But Dr. Kuznets' use of the two terms "net national product" and "national income" as identical, his assertions regarding basic procedure in preparing national income estimates, his judgments regarding what commodities and services are properly included in the product of the nation's economic system, and his methods of evaluating some of these commodities and services, will not be so uniformly accepted.

In a footnote Dr. Kuznets remarks: "we conceive national product properly as a sum of values rather than identify it improperly with a congeries of specific commodities and services." To the reviewer it seems an exceedingly unfortunate use of language to apply two terms synonymously to one concept and to deprive thereby another equally important concept with an especially descriptive and apt term. It is preferable to use the term "national product" to designate the congeries of specific commodities and services resulting from economic activity, and the term "national income" or "value of the national product" to designate the sum of the values of those commodities and services.

Dr. Kuznets' explanations of his basic procedure in preparing estimates of the national income are ambiguous, if not in fact contradictory. In one place he states: "whatever results of economic activities were included in the national product totals were assigned the value they brought on the market." In this statement the pronoun "they" presumably refers back to the word "results"—implying that the final sale values of consumers' commodities and services, and of new capital items, were estimated and summed to obtain the value of the national product. In another place Dr. Kuznets states that the available data render it necessary to estimate national income by the procedure of aggregating the incomes paid to individuals plus net savings of all business enterprises. As a matter of fact, Dr. Kuznets uses only the second of these two in estimating the total national income, but he also uses the first method in estimating the value of capital items and of consumers' commodities. By this combination of methods he obtains an evaluation of consumers' services not embodied in commodities as a residual difference between the total value of the national product and the value of that part of the product which consists of commodities.

It is greatly to be regretted that Dr. Kuznets did not follow both methods,

keeping them as independent of each other as possible and comparing the results. His assertion that the available data require the use of the second method in estimating the total national income is sheer nonsense. Data regarding the value of consumers' services not embodied in commodities are as plentiful and as reliable as those regarding "entrepreneurial withdrawals," "net savings" of unincorporated business enterprises, or wages and salaries in some industries.

With reference to the character of the individual and social activities which are included in the "nation's economic system," Dr. Kuznets states:

> It (national income) refers, by design, to the net product of the economic system, which, for economically advanced nations of recent times, may be treated as identical with the market economy. If "market" is understood broadly as the meeting place of all buyers and sellers, no matter how greatly freedom of transactions may be curbed by custom or by regulation, then national income measures the net product of activities connected through the market, and excludes the results of other activities that may supply utilities but are outside the market mechanism.

Dr. Kuznets, however, does not adhere strictly to this position, and it may be suggested that he would have expressed his own position more accurately had he been less categorical in his statement regarding the identity of the market economy and the economic system. In fact, Dr. Kuznets makes several important departures from this assumption in his estimates of national income. These departures, as stated by himself, are as follows:

> There are a few types of income that do not pass through the market and do not find their equivalent in the sphere of monetary circulation, but that should nevertheless be included as representing the result of economic activity: (1) the share of the product retained by entrepreneurs for their own consumption (largely on farms); (2) payments in kind to employees (a practice especially important in compensating farm laborers, domestic servants, steamer crews, etc.); (3) services of houses inhabited by their owners.
>
> However, not all compensation to individuals for marketable activities is included . . . In general, the national product totals presented below exclude the returns from three sources: (1) activities that have been explicitly recognized by society at large, overtly in the form of legal prohibition, not only as unproductive but also as distinctly harmful: theft, robbery, prostitution, murder, smuggling, counterfeiting, forgery, drug peddling; (2) activities that while legal, represent largely shifts of income among individuals rather than additions to the command over goods: gambling profits and speculative gains from the sale of assets by non-professional groups that can in no way be interpreted as resulting from skill in the performance of any useful functions that speculative markets may be presumed to render; (3) gains and losses that result from a mere change in the price level, on the same grounds that apply to the exclusion of speculative gains resulting from professional activity.

Dr. Kuznets also includes in the national product one important group of services not specified in this quotation which are likewise not distributed

through the market economy, namely, those furnished by governments to individuals without specific charges.

The reviewer concurs with Dr. Kuznets as to the necessity of making some departures from the assumption that the economic system is identical with the market economy. In at least two respects, however, Dr. Kuznets' procedure in making such departures is such as to produce serious doubts about the year-to-year comparability of his estimates of national income. (1) If Dr. Kuznets has, as he states, actually excluded the returns or income derived from activities legally prohibited, then his estimates for the years 1933–35 are raised by several per cent, as compared with those for preceding years, just because of the repeal of the 18th Amendment and of accompanying State laws, in addition to whatever differences result from changes in prices and consumption of alcoholic beverages or from changes in the earnings of persons engaged in their production and sale. (2) Dr. Kuznets has valued governmental services rendered to individuals by the amount of direct taxes paid by persons out of their individual incomes. (See *Studies in Income and Wealth, Vol. I*, p. 237. Dr. Kuznets does not explain in *National Income and Capital Formation*, except by inference, how he handled such services.) Because of the marked differences between the yearly variations in such taxes on the one hand and the yearly variations in the volume and cost of Government services on the other, and the fact that Dr. Kuznets does not allow for such variations in his indexes of retail prices, serious distortions occur in the comparability of Dr. Kuznets' annual totals, particularly those given in "constant" or "deflated" prices.

In view of the growing importance of governmental services and of the extent to which these services are financed by indirect taxation, and of the complexity of the problem of treating them in national income measurements, it is perhaps desirable to illustrate by a simple example the nature of the distortion produced by Dr. Kuznets' procedure. Suppose that some branch of the cosmetics industry has in one year an output of 60,000 units which are sold at a market price of $5 each, that no taxes of any kind are paid by this industry, and that the entire proceeds from the sale of the product are paid out in some form of individual income. Suppose that in the next year a tax amounting to $1 per unit is levied on the industry, the proceeds of which are used to pay the salaries of additional teachers in the public schools. Consider the following three extreme cases with respect to the effect of the tax upon sales and upon individual income and note how estimates of national income are affected thereby.

Case I. Market price of $5 per unit is maintained, sales are unchanged, but proceeds from sales distributed to individuals are reduced by the amount of the tax.

Case II. Tax is wholly passed on to consumers by advancing the market price to $6 per unit, and the amount of proceeds from sales distributed to individuals remains unchanged.

Case III. Market price is advanced to $6, sales are maintained in dollar amount but drop in physical volume from 60,000 to 50,000 units. Since taxes absorb $1

per unit, individual income drawn from the industry is reduced from $300,000 to $250,000.

The results of the imposition of this indirect tax, using Kuznets' procedure, are shown in the table below. Since national income in "constant" prices is in essence merely a device for deriving an index of production of the commodities and services constituting the net national product, the last column of the table reduces income in "constant" prices to such an index.

	Income derived from (in Kuznets' language, "originating in") the specified parts of		Total income from these activities in current prices	Index of retail prices	Total income in constant prices	Index of production
	Cosmetic industry	Government				
Case I:						
Prior to tax	$300,000	—	$300,000	100	$300,000	100
After tax	240,000	$60,000	300,000	100	300,000	100
Case II:						
Prior to tax	300,000	—	300,000	100	300,000	100
After tax	300,000	60,000	360,000	120	300,000	100
Case III:						
Prior to tax	300,000	—	300,000	100	300,000	100
After tax	250,000	50,000	300,000	120	250,000	83

A method which shows income in constant prices, or an index of production, to be unchanged when educational services are increased without impairment of other output (Cases I and II) and which shows income in constant prices to have declined when the real change was a shift from one kind of product to another (Case III) is a method which the reviewer finds it difficult to commend.

Another aspect of Dr. Kuznets' treatment of governmental services, that of handling government deficits, produces astounding fluctuations in his estimates of the segment of the value of the national product originating in government. He estimates, for example, that $923 million of income was derived from government in 1919 and $6,906 million in 1920. In 1930 and in 1932 the amounts were $8,184 million and $5,859 million respectively. These amounts are presumed to include not only the value of services rendered to individuals (assumed to equal the amount of direct taxes paid) but also the value of services rendered by governments to business enterprises and embodied in the prices of the products of such enterprises and, in addition, the value of accretions to the assets of governments. How all governmental services could reasonably be valued as worth seven times as much in 1920 as in 1919—or valued in 1932 at less than three-fourths of what they were worth in 1930, in view of the very slight reduction, if any, in their volume and cost—is to the reviewer quite inexplicable.

The explanation of Dr. Kuznets' curious evaluation of income originating in governmental activity is the fact that he has treated a government deficit

as identical in character with a definitely ascertained loss by a business enterprise. Is not, however, a government deficit more closely analogous to goods sold on credit? If a government borrows money in order to finance services to its "customers," with an acknowledged and legally enforceable obligation upon the buyers (taxpayers) to pay at some future date—is not this essentially similar to a merchant who borrows from his bank in order to finance accounts receivable? Perhaps Dr. Kuznets is anticipating a repudiation of obligations by taxpayers and is consequently treating those obligations as bad debts to be written off at once.

Having used the dubious criterion of "cash" versus "credit" sales (that is, tax collections versus borrowings) as an element in the estimation of the "market value" of governmental services in current prices, and obtaining thereby an annual series of figures showing curious fluctuations, Dr. Kuznets proceeds to convert the income totals in which they are included into income at "constant" (1929) prices by applying the Bureau of Labor Statistics' index of wage-earners' cost of living. The significance of this deflated series is entirely beyond the comprehension of the reviewer.

Dr. Kuznets' method of handling the evaluation of changes in inventories will probably also provoke considerable discussion among economists working in the field of national income, for this problem, like that of the evaluation of governmental services, is highly controversial. The reviewer does not feel competent to comment upon Dr. Kuznets' procedure in dealing with this problem.

The segments of the national income regarding which Dr. Kuznets' procedure is of doubtful validity are important. They constitute, however, only a minor part of the total income of the nation, and for this reason the figures Dr. Kuznets presents probably show more accurately than any series of figures previously available the year-to-year changes in the value of the gross and net national product during the period 1919–35.

The chief value of national income estimates, apart from their use as an index of physical production of all commodities and services during successive time periods, lies in the relative importance of the various component parts and in the relative changes from year to year in the values of these component parts. Dr. Kuznets presents three classifications of such component parts, namely: (1) industrial origin, (2) type of income, and (3) character of consumers' outlay and of capital formation. He also gives a classification of type of income by industrial source. The categories used in the three classifications, with slight rearrangements made by the reviewer, together with average percentages of the value of the net national product during the entire period 1919–35, are shown on the following page.

The value of durable capital goods consumed in the process of production amounted, for the entire period, to 13.5 per cent of the national income, so that the value of the gross national product exceeded by this percentage the value of the net national product. Only 38 per cent of the value of gross capital formation—including all construction and durable producers' commodities, and all changes in business inventories, stocks of silver and gold,

Industrial origin		*Type of income*		*Consumers' outlay and capital formation*	
Commodity-producing industries—total	*57.1*	*Employees' compensation*—total	*69.4*	*Consumers' outlay*—total	*91.7*
Agriculture	9.7	Wages and salaries	69.2	Perishable commodities	37.0
Mining	2.4	Relief, pensions, and compensation for injuries	0.2	Semidurable commodities	15.8
Manufacturing	21.8	*Entrepreneurial and property income payments*—total	*29.7*	Consumers durable commodities	10.8
Construction	3.2	Withdrawals by entrepreneurs	12.1	Services not embodied in new commodities	28.1
Commodity-handling industries—total	*24.0*	Rents	4.6	*Net capital formation*—total	*8.3*
Transportation	7.6	Dividends	5.8	Business	3.6
Light, power, and communication	2.7	Interest	7.0	Residential construction	0.8
Trade	13.7	Net balance of international payments	0.2	Public agencies	3.2
Service industries—total	*38.1*	*Not distributed to individuals*—total	*0.9*	Net change in claims against foreign countries	0.7
Banking and insurance	2.7	Business enterprises	0.4		
Real estate	7.9	Government	0.5		
Government	10.9				
Service	13.1				
Miscellaneous	3.5				
Unallocated adjustments	*0.8*				

and claims against foreign countries—represented net capital formation. Over 60 per cent represented replacements of structures and durable goods which depreciated or were entirely used up in the process of production.

Some of the more important conclusions which may be drawn from the classified estimates for the various years are as follows:

1. The percentages of national income derived from the commodity-producing and commodity-handling industries declined substantially during the period 1919–35, while the percentage derived from the industries producing services not embodied in commodities increased greatly.

2. The percentage of the national income received by individuals in the form of employees' compensation increased somewhat, while the percentage received in the form of entrepreneurial and property income payments, and also the percentage not distributed to individuals, declined.

3. Net capital formation averaged over 12 per cent of national income during the period 1919–29. During the years 1930–35, however, this figure was negative.

4. Consumers' outlay for durable and semidurable commodities decreased in relative importance during the latter part of the period. Outlay for services not embodied in commodities presumably increased in relative importance, but the derivation of the value of such services as a residual, together with the method of evaluation of governmental services, makes any conclusion regarding their value extremely hazardous.

Since *National Income and Capital Formation 1919–1935* is a summary volume, with details of methodology and sources of data omitted, its purpose is presumably that of presenting the conclusions of elaborate economic investigations to economists, government officials, legislators, educators, business men, and other interested persons among the general public. For this purpose the book has several serious defects. (1) The general arrangement and order of presentation of the material, although logical, is such as to be extremely confusing to all readers except possibly a few dozen specialists in the

field. Readers are plunged immediately into a technical and abstruse discussion of definitions and terminology. Most of them will reach the first table, showing yearly totals of the value of the gross and net national product, with a very confused impression of what the figures represent. (2) Long paragraphs, in which conclusions drawn from the estimates are mixed with technical explanations of procedure, make reading a tiresome and tedious task. (3) Some of the terminology is inept, and important statements are sometimes inaccurate or ambiguous. Two examples, other than those previously mentioned, may be cited. The terms, "net savings of business enterprises" and "net savings of governments" are used to designate income not distributed to owners or employees; "undistributed income" or "undistributed earnings" would be more accurate and less confusing in view of the numerous ways in which the word "savings" is already used. In the statement "a large share of total national income arises from activities that do not constitute either production or handling of new commodities; on the average, these account together for slightly over 61 per cent of national income," the antecedent of the pronoun *these* has been confused; comparison with the tables shows the correct percentage to be 39. (3) Some of the innovations in printing style, particularly the absence of indentations in the run-over lines of tables and in footnote paragraphing, needlessly enhance the strain on the eyes.

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The Floating Debt of the Federal Government, 1919–1936, by Edward Raguet Van Sant. Baltimore, Maryland. The Johns Hopkins Press. 1937. The Johns Hopkins University Studies in Historical and Political Science. Series LV. Number 4. xiv, 88 pp. $1.00.

This monograph offers a brief history of the experience of the United State Treasury with short term securities from the close of the World War to the end of the fiscal year, 1936, with an illuminating discussion of the effects of such operations on the money market and banking system. The dangers which governments must face if they finance their credit needs through increases in the floating debt have been treated with considerable clarity and restraint. The author's conclusions are not without point in a period such as the present when the amount of United States Treasury obligations maturing within six years is larger than the total debt at the close of 1930.

Murray Shields

Irving Trust Company

Trends in Relief Expenditures, 1910–1935, by Anne E. Geddes. Washington, D.C.: Division of Social Research, Works Progress Administration. 1937. Research Monograph X. xvii, 117 pp.

This monograph contains heretofore unassembled data about relief expenditures in the United States prior to the time the Federal Government

began to concern itself with relief statistics. Information collected by the United States Children's Bureau since 1929 and by the Federal Emergency Relief Administration in 1933, 1934, and 1935 is discussed in the light of this earlier material. Conclusions believed to be implicit in the data have been indicated.

Data about relief expenditures before 1929 are rare. The available information has been widely scattered in fragments of varying significance. The legal fiction that relief was a matter of local concern deterred the systematic collection of statistics about relief on a state or national basis. The fallacy that depression "emergency relief" was unrelated to earlier "poor relief" and the high pressure under which emergency relief agencies operated prevented the majority of administrators from searching archives and ledgers to compile such data *ex post facto*. Elsewhere the casualness with which some communities had accounted for public assistance expenditures made such searches worthless. Now, however, the premise from which this monograph proceeds is all but universally recognized, that "emergency relief operations . . . can be viewed in proper perspective only against a background of previous relief experience in the United States."

The method used in this study to provide such a background was to collect as many reliable fragments of information about predepression relief expenditures as could be located. Each fragment was analyzed as an entity. The results of these analyses were then assembled in order that the experience so indicated could be compared.

Some of the fragments are sizable. Data indicating the annual expenditures of New York State and for New York City were available for each year from 1910 through 1934. Similar data were available for Indiana. In addition, there were data for New Haven, Conn., from 1910 through 1925. From the Census Bureau's *Financial Statistics of Cities* it was possible to ascertain the expenditures of a representative group of 16 key cities, in the occasional years between 1910 and 1935 for which these reports were compiled. The findings of a number of experiments in the uniform reporting of social statistics were also utilized. Since 1929, the uniform registration of social statistics by the United States Children's Bureau has provided a picture of relief operations in 120 more or less representative urban areas. For 1933, 1934, and 1935, the reports made to the Federal Emergency Relief Administration furnish almost complete material on both urban and rural relief expenditures.

What may perhaps be called the core of the monograph is the chart on page 46, "Trends of Expenditures for Public Outdoor Relief in Selected Areas, 1910–1935." In this chart, the evidence derived from each of the statistical series previously discussed in the text has been plotted in comparable terms. The similarity of experience thus revealed provides a more than adequate foundation for the conclusions to which the study points.

These conclusions, phrased in unassuming and carefully qualified language, seem to revolve around this major point, that at least since 1910 there has been a strong underlying upward trend in relief expenditures. The

precipitous increase since the depression has been but a sharp acceleration of a tendency manifest through the two preceding decades. In these decades, in urban areas if not generally, the rate of increase in relief expenditures exceeded the rates of increase in population growth and in the cost of general government. The enactment of legislation to provide special treatment of certain classes of dependent persons apart from the mass of the relief population widened the base of governmental responsibility to include state and later Federal participation but failed to arrest the increase in the cost of the residual relief functions left with the traditional poor law authorities.

The study is most modest. It does not place any more strain on the data than they can plainly support. It might be possible to argue that it errs on the side of understatement. Yet this understatement is intentional, and the study itself points to many if not all the phases of the data where this is done. In the absence of data on the trends in the size of the relief population, the monograph refuses to comment on the reasons for the increasing relief expenditures in times of apparent national prosperity. The study properly avoids any suggestion that even a jerry-built estimate of national aggregate relief expenditures from 1910 on might be made from the material used to point out probable trends. The rigid discipline by which interpretation is held to a conservative minimum enhances the reader's confidence in the strength of the monograph as an account of trends in relief expenditures during the years with which it deals.

And no thoughtful person, statistician or layman, can leave the monograph without feeling grave concern over the evidence it presents that during the nation's era of most lavish prosperity, relief expenditures were climbing with ominous celerity.

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The Chain Store Problem, by Theodore N. Beckman and Herman C. Nolen. New York and London: McGraw-Hill Book Company, Inc. 1938. viii, 350 pp. $3.50.

The authors of this book, feeling that most discussions of the advantages and disadvantages of chain stores had been partisan and unscientific, set out to make a "critical and comprehensive analysis" of the so-called chain store problem "in an unbiased and strictly impartial manner." Their task was not simple. The arguments to be evaluated were numerous, complex, and often highly colored. Satisfactory evidence was scarce.

The resulting book catalogs the advantages and disadvantages of chains, lists many of the arguments presented by proponents and opponents, and brings together many pertinent facts. That manifestly is a service to students of the subject. It is unfortunate, however, that the book was not edited more carefully. The style frequently is flowery; there is a tendency toward both redundancy and repetition; and there are minor mistakes. In many instances, greater restraint would have been desirable. Partly because

of this lack of restraint, some readers will infer that the authors were not strictly neutral but vacillated in their point of view, being first pro-chain and then anti-chain. The index is inadequate. Such words or phrases as the following are not to be found: supermarket, variety chain, limited price variety chain, Indiana, Supreme Court, Woolworth, Kresge, Newberry Grant, National Association of Food Chains, agriculture, farmers, and agricultural surpluses.

Again, one cannot accept unqualifiedly the authors' disagreement with the Bureau of the Census regarding the definition of a chain. Beckman and Nolen strongly favor treating any firm with two stores or more as a chain. This may be logical, but is somewhat unrealistic. Also, in citing the operating expense statistics published by the Bureau of the Census, or based on the Census data, the authors could have indicated specifically that the definitions adopted by the Census, although presumably necessary, resulted in final figures which understate the actual level of expenses in retail trade.

Beckman and Nolen conclude that the chains have made such important contributions to the efficiency of distribution and to the reduction of retail prices that to outlaw them is quite unthinkable. On the other hand, the authors say that the chains should not be allowed to develop free of all controls and discriminatory legislation. To quote from page 294, "there is, for example, the possession of monopolistic power in some cases and a definite trend toward monopoly in the chain system as a whole. Then there are the bad effects of absentee ownership and control, a tendency to lower wage standards and limiting the employment opportunities of white-collar workers, and a strong tendency to stifle individual economic opportunity. Above all, there are certain abuses of which chains have been found guilty, such as clubbing manufacturers into giving them unwarranted discounts and allowances of various kinds. Similarly, it is claimed that chains have been escaping their just share of certain taxes and community obligations."

Professors Beckman and Nolen believe, however, that, even where the chains are strongest, conditions do not call for governmental regulation of the type imposed upon the railroads and public utilities. They come out clearly in opposition to any attempt to offset or destroy the economic advantages, or to curtail the growth, of the chains. They suggest only that any abuses which have developed in the chain store system, and any "inequities" favorable to the chains, should be eliminated. The Robinson-Patman Act is endorsed as an effort to do away with the former, and scientifically devised taxes are suggested as a possible means of correcting the "inequities."

There can be little quarrel with these conclusions in principle. One may doubt, however, whether workmanlike research will disclose "inequities," as distinguished from economic advantages which should be preserved, sufficiently great to warrant substantial discriminatory taxation.

Approximately one-third of the book is devoted to reports, not previously published, upon a study of chain and independent stores in Florida which was conducted by Professor Beckman in 1935. One portion of this study had

to do with comparative prices of drugs, dry goods, and automobile accessories, as well as foods. Most such studies have been limited to foods. Another portion of the study was devoted to the measurement of "consumer buying-preferences," or patronage habits and motives, in foods, drugs, shoes, and women's apparel by income groups and, to some degree, by cities.

The entire Florida project was notable for the careful manner in which it was planned and carried out and for its wide coverage. It is well that a description of this piece of work and its findings finally is available in print. Many readers may refer to the present book more frequently for its report on this survey than for the broader discussion from which its title is derived.

Appendices provide convenient summaries of state laws imposing taxes on chain stores (as of July 1, 1937), of court decisions relative to such laws, and of city ordinances taxing chain stores. There are helpful chapters on chain store taxation and on other legislation affecting chains.

Although statistics are introduced from time to time, the use of these data gave rise to no complicated problems of method.

In all our thinking about the chain store's problem we should remember (a) that, to a large degree, this problem centers upon the food chains and (b) that the retailing of food is one of the more highly dynamic segments of our economy. During the last 25 years we have seen the old service wholesaler-service retailer system displaced extensively by the chain of small or neighborhood stores; we have seen the chain of this type transformed in important measure into the chain of combination stores with neighborhood stores as satellites; and now, at least in urban areas, we see the self-service supermarkets, with expense rates 20 per cent to 25 per cent smaller than those of the regular chains, cutting into the business so markedly that the chains themselves are beginning a second transformation by the opening of supermarkets.

The chain store problem became acute, and the states got around to strong anti-chain taxation, directed largely at the food chains, at about the time when economic evolution had decreed the reformation or decline of these chains. Presumably the supermarkets today constitute a much greater threat to the peace and prosperity of the food chains than any taxes now in effect or likely to be levied in the near future. The supermarkets, of course, are not adapted to the needs of extremely small communities and, even in the cities, they should not be expected to displace all the small neighborhood stores, independent or chain. Nevertheless, a realignment of interests is likely to take place. Chains which have no supermarkets shortly may find that their point of view closely resembles that of the smaller independents; and the two may combine to attack the supermarkets, whether independent or chain-owned. If punitive taxes are to be employed, a way may have to be found to vary these taxes on the basis of sales per unit rather than on the number of units; and attention may focus somewhat more on efforts to prevent price cutting than is the case today.

Considerations of this type received too little emphasis in the book under review. The discussion which it presents relates chiefly to a static condition;

and there is little indication in the book that this condition is about to change. Of course, this criticism is not so serious as some readers may infer, for many of the arguments advanced, both pro and con, with reference to chain stores as we have known them, will apply, perhaps with more force, in the discussions of the new problem on which the changes now under way will focus attention.

In addition, the chain store problem has been, and still is, of widespread interest and importance. Discussion of this problem has not been as calm and as factual as one might desire. This new book by Beckman and Nolen, despite its shortcomings, represents a worth-while addition to the literature of the subject.

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Social Insurance Legislation and Statistics, A Study in the Labour Economics and Business Organization of Neo-Capitalism, by Benoy Kumar Sarkar. Calcutta: The Calcutta Publishers. Selling agents—U. Ray & Sons, 117–1, Bowbazar Street, Calcutta. 1936. xxi, 446 pp. Rs. 8/–.

This is at least as much a tract as a book on social insurance statistics and laws. It devotes, it is true, more than half its pages to these subjects, but this part of the book is quite clearly only documentation for the main argument. The argument is that the time has come for India, following the rest of the industrialized world, to adopt social insurance.

We are not unacquainted in this country, just emerging from the deliberative into the pioneer stages of social insurance, with books that succeed fairly well in being both scientific and partisan. But America has never produced so strange a combination of exhortation and figures, preaching, and facts. This for a variety of reasons. The world, says the author, has entered the second industrial revolution: its leading characteristics are "technocracy" (the European "rationalization"), "trustification," and rapidly expanding state control of business. A collateral characteristic, and one that Sarkar sees related very intimately to these others, is the world-wide spread of social insurance. The neo-capitalist, the capitalist of the twentieth century, has been "to a certain extent perhaps chastened by the impact of the working classes and the community in general" (p. 77). Social insurance is one of the results. But India is barely on the threshold of her *first* industrial revolution. The employing class is still very much in the saddle, the state stands still on the sidelines; most important, neither labor nor community is socially minded. Organized labor is still the exception, and the few unions are not strong. The community tends to think in terms of the putative glories of a Golden Age of the long past, in the forms of a caste system that has shaped India's life and thought since the dawn of man.

The author, that is, has set himself an impossible task: to shake his countrymen out of their ancient lethargy, force them to skip at least a century in industrial history and no one knows how many centuries of social

and political history, jump straight into social insurance, essentially a twentieth-century "improvement." "Social assurance is not yet a question of practical politics;" in the meantime he is appealing to the two groups he is convinced hold the key to India's future: the workers and the clerks. It is significant of India and of the nature of the author's task that he feels he needs to devote one chapter to "The Dignity of the Clerical Service," one to "The Nation-Building Functions of Clerks." The long and often dull pages of statistics in the middle of the book are for "students of statistics, economic history and political developments." The tocsin, delivered at the beginning and end of the book, without statistics, is for the underprivileged. The pitch of the latter may be judged by the author's statement that there is in India an "unreasonable gospel of hatred" against clerical service, that even "gazetted officers in government service have learnt to consider themselves as almost worthless human beings" (p. 94).

One other fact makes the nonstatistical sections of the book less than completely logical and orderly to the Occidental mind. It is the apparent necessity of making both a general economic and a specific case for social insurance. Marshall and John Stuart Mill, not to mention Pareto and Sorokin, are adduced to prove that social insurance costs can be paid and that social insurance benefits are an addition to and not a substitute for wages. On this point Sarkar is himself not very clear. He seems to accept the iron law of wages on page 427; on pages vii, 408, and 412 he concludes that the needs of the worker "can therefore be satisfied to a certain extent and within certain limits by the provision of wages at rates which are beyond those dictated by free competition and the iron law" (p. 428).

It is not surprising, therefore, to find that Sarkar has no detailed Indian social insurance program. Though he chides Indian workers with fanning the air with programs so broad they never get down to details like wages and specific social insurance plans, he really gets not much closer. He urges the middle class to hasten the industrialization of India because labor upheaval will help them (it seems the other way about to the American and European); he opposes state contributions to social insurance because they are a dole to the capitalist; his most specific recommendation is that the clerks ally themselves with the manual workers. He strongly suggests that labor unions are essential for social insurance administration, although neither British (except for health insurance and then to a minor degree) nor American experience indicates that this is so. I think he quite misunderstands the experience of the entire world when he says that "the business aspect (of social insurance) has too long escaped the attention of the insurance companies" (p. 407). Social insurance is not private insurance, and mischief generally follows when they are confused.

The statistical and legal materials are mainly to provide social insurance and related comparisons for the leading industrial countries, particularly Germany the pioneer and Great Britain the homeland. The sources are generally secondary, too often out-of-date. When available, Indian materials are given. The feel of this part of the book is conveyed in the author's

reference to his comparative statistics on public finance. "We are neither interested in the discrepancies of estimate for the present nor in the scientific comparison between country and country."

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Profit Sharing for Wage Earners, by C. Canby Balderston. New York: Industrial Relations Counselors, Inc. 1937. viii, 157 pp. $2.00.

In the never-ending search for an easy formula for harmonious relations in industry, the device of profit sharing has been rediscovered and hailed anew at almost every upturn of business for the better part of a century. During the 1920's however, much of the enthusiasm for profit sharing was diverted to employee stock ownership. Following sad experiences with this twentieth century substitute, interest in the older form of making "enterprisers" out of wage earners revived in 1936 and early 1937. Despite the succeeding reversal in business, the recent Congress became sufficiently impressed by the promises of industrial peace and prosperity advanced by the advocates of profit sharing to appropriate $30,000 to study it further.

With Professor Balderston's excellent monograph at hand, Congress might well have saved the country the cost of another survey. In 68 pages of text and a 76-page appendix, Professor Balderston concisely summarizes the history of profit sharing, its objectives, the factors affecting success or failure in its use, the structure of plans, and experience under them. In a final chapter he outlines the conditions under which a profit-sharing plan might be justifiable. The appendix includes verbatim statements of three active plans of a type which the author approves as well as tabular summaries of a large number of other American and British plans. Many carefully prepared tables and charts supplement the text.

Although the author concludes "that profit sharing has real merit," he states that extreme care must be exercised to determine the suitability of the particular situation and the soundness of the plan in relation to it. Standing alone, he holds, profit sharing is a weak device. It should be the culmination of a well-rounded personnel policy under which properly financed plans protect employees in times when earnings decline or disappear. The author recommends that any share in extra profits be deposited in a fund to strengthen or liberalize such protective programs rather than disbursed as cash bonuses. With ample provision against the hazards of old age, unemployment and sickness, he believes, profit sharing should be used to subsidize employee savings.

The author would probably agree that scarcely a dozen companies in the country are justified in inaugurating a profit-sharing plan in the face of these requirements. Rather than an easy formula, profit sharing becomes under this prescription the cornice of an elaborate and costly structure of protective programs. Even then there seem to be grave doubts as to its advantage compared to a farsighted policy of payroll reserves and employment stabili-

zation. Professor Balderston indicates the failure of profit sharing as an incentive, its tendency to accentuate fluctuations in earnings, and the objections raised against it by organized labor. With a high mortality rate in the past and a necessity of sustained nursing in the future, it appears that profit sharing is too delicate a brain child to resist the rigors of industrial relations today.

Underlying the practical difficulties of profit sharing is the fundamental confusion of concepts inherent in the device. The capitalistic system is based on the principle that profit is the noncontractual reward of successful enterprise, whereas wages are the compensation of workers who give up the chance of uncertain gains in exchange for a more stable income. As an economic system, capitalism is accepted in a democratic society so long as it serves to protect and enhance the livelihood of the great mass of citizens. Rather than a greater sharing of risk, our people seem to be seeking a greater measure of security. Although incentive remains an essential prerequisite to continued progress, as well as to security itself, our industrialists will need to find more constructive means of stimulating incentive than adding to the uncertainty of wage-earner incomes. Professor Balderston's monograph, although perhaps a little too considerate, serves clearly and effectively as a warning to thoughtful industrial executives.

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Princeton University

British Methods of Industrial Peace, A Study of Democracy in Relation to Labor Disputes, by Ducksoo Chang. New York: Columbia University Press. Studies in History, Economics and Public Law, Number 425, Edited by the Faculty of Political Science of Columbia University. 1936. 332 pp. $4.25.

Out of this excellent, first-hand study of labor relations and government attitudes toward collective bargaining in Great Britain during the past century or so come a number of conclusions which are of prime significance to all Americans who are looking for a road to industrial peace in the United States. On the basis of his survey of prewar, war, and postwar experience in England as a whole and in five important industries (coal mining, railways, iron and steel, engineering and shipbuilding, and cotton textiles), the author finds (1) that labor peace based on industrial democracy—i.e., on good-faith recognition of unions by employers—is the only enduring peace in a free country dedicated to political democracy; (2) that most labor issues can be settled voluntarily, without government intervention, through collective bargaining, based on the will-to-agree, between strongly organized unions and employers; (3) that, nevertheless, when the collective bargaining process breaks down (as it occasionally does over major issues), government should be ready, with agencies for mediation and voluntary arbitration, to step in and help the disputants find a common, face-saving basis for settlement; (4) that compulsory settlement of labor disputes by government agencies is not

desirable in a democracy; and (5) that under "voluntary" government intervention there will be, rather than the application of rigid rules or principles of settlement, the making of decisions based on expediency and on the relative economic strengths of the disputants.

CARROLL R. DOUGHERTY

University of Pittsburgh

Newspapers and the News, An Objective Measurement of Ethical and Unethical Behavior by Representative Newspapers, by Susan M. Kingsbury, Hornell Hart, and Associates. New York: G. P. Putnam's Sons. Bryn Mawr College, Series in Social Economy Number One. 1937. xi, 238 pp. $2.50.

The authors of this study originally undertook to make "a comprehensive and scientific investigation" into the ethical standards which govern the handling of news and advertising by American newspapers. In practice, the scope of the survey was considerably narrowed. It still represents a highly ambitious attempt, however, in that it endeavors to evaluate the ethical standards of some 44 "representative" newspapers by the use of quantitative measures.

The evaluation of individual newspapers unfortunately has little current value, since it is based chiefly upon an analysis of front pages published during July, 1929. In the intervening years, some of these newspapers have gone out of existence and others have changed ownership. All of them have been subjected to the severe strains of a prolonged depression. One can only conjecture whether these circumstances have made any substantial changes in the ethical standards of the papers concerned.

For the readers of today's newspapers, interest in the study must, therefore, center upon the methodology used. This consists primarily of working out a series of ingenious indexes of ethical and unethical behavior which carry such names as the spectrum of news interests, the index of newspaper bias, and the index of pernicious medical advertising. The basic measure is usually the space given, especially in headlines, to various types of news or advertising, opposing sides in a controversy, undesirable advertising, and the like. No attempt is made to set up an ideal or absolute standard. The authors content themselves with working out "representative" figures for actual newspaper practice and grading individual newspapers according to the extent to which they fall above or below the "representative" level. It is evidently the hope of the authors that their indexes can be computed periodically and that the managements of individual newspapers can be induced to improve their practices in areas where they are graded relatively low.

The present reviewer cannot accept wholeheartedly the faith of the authors that their work is completely "objective" and "scientific." For example, throughout the study, both in mapping out procedures and in applying them, it has been necessary repeatedly for the staff to make conscious or unconscious judgments of qualitative values. It is quite doubtful whether another staff, given the same materials and a description of the procedures,

would come out with identical results. Again, the analysis of readers' interests is quite arbitrary and incomplete, and the authors give no convincing arguments to support their belief that an appeal to some kinds of interest is "ethical" and an appeal to other kinds of interest is "unethical." Yet again, the space given to different kinds of news is not always a good indicator of newspaper policy. An important news "story," such as a national election, the flight of Lindbergh to Paris, the kidnapping of the Lindbergh baby, the abdication of Edward VIII, the destruction of Shanghai by Japan, and the annexation of Austria by Germany, will capture the front page of practically all newspapers. The important test here is not so much the space allotted as the point of view of the reporters, in newspaper parlance "the angle which is played up." Similarly, the unequal allotment of space to opposing sides in a controversy may result not from bias so much as from the fact that one side makes an effective presentation of its case whereas the other side is dull and ineffective, or that one side is represented by conspicuous and colorful spokesmen as against obscure, colorless opponents, or that one side presents its case on a day when news is scarce whereas the other picks a day when competition for the front page is severe.

Despite the shortcomings indicated by these examples, the study is an interesting attempt to deal by the use of quantitative measures with a difficult and important subject. As such it deserves sympathetic attention from everyone concerned with the problems of newspaper ethics.

The foregoing comments apply to the first two-thirds of the study. The last third lists and describes previous "objective" studies of newspapers and presents an admirable bibliography. Although it will be of less general interest, this section will be indispensable to future students in the field.

Reavis Cox

University of Pennsylvania

Land Utilization in China, A Study of 16,786 Farms in 168 Localities, and 38,256 Farm Families in 22 Provinces in China, 1929–1933, by John Lossing Buck. A Report in the International Research Series of the Institute of Pacific Relations. Published by the University of Nanking. Chicago: University of Chicago Press. 1938. Vol. 1, *Text*, xxxii, 494 pp. $5.00. Vol. II, *Atlas*, xii, 146 pp. $5.00. Vol. III, *Statistics*, xv, 473 pp. $10.00. The Set $15.00.

The appearance of Dr. John Lossing Buck's *Land Utilization in China* is a major event in our understanding of Chinese life. Whereas almost all previous studies of the rural scene have necessarily been based on unsupported generalizations, we now have a wealth of first-hand statistical data carefully collected, painstakingly analyzed, and attractively presented.

These three volumes are the result of a decade of field study throughout China south of the Great Wall. The work has been carried on in the College of Agriculture and Forestry at the University of Nanking, aided by liberal grants from the Institute of Pacific Relations and various official Chinese

organizations. Scores of Chinese and foreign scholars have been associated with the project.

Land utilization is interpreted broadly. The statistical study includes all aspects of agricultural economics as well as population and nutrition problems. Excellent descriptions in Volume One also deal with the geographic problems of topography, soil, climate, and land resources. Unfortunately there are few data from Manchuria, owing to the hostilities of recent years. The appearance of the study at this time is particularly fortunate, since it may be many years before a similar survey will be possible. The implications as to China's welfare and economic possibilities are fundamental.

Dr. Buck's study is based upon field work in 168 representative localities in 22 of the 28 provinces. Each of the 13 regional investigators was a graduate of the College, and local investigators were carefully trained. Detailed schedules were filled out for one hundred farms in each area. On the basis of the data collected the country was divided into eight agricultural regions. The north is characteristically wheat land, and the south is rice land.

Agriculture in China is less efficient than sometimes thought. Large amounts of labor are applied to the fields, but the yields are not commensurate. Rice harvests per acre are higher than in Japan but less than in the United States. Wheat yields are about the same in China and the United States, but, in general, crop yields are less than elsewhere. The production per man-equivalent (one adult farmer working a full year) is 14,000 kilograms in China compared with 20,000 kilograms in this country. Whereas it requires 26 man-equivalent days to raise an acre of wheat in China, 1.2 days suffice in the United States. Taxation averages four times as much per acre as in the United States.

Detailed population analysis shows an average rural density of 1,500 per square mile of land under cultivation. The birth rate appears to be at least 38 per 1,000, with a death rate of 27. The density and rapid increase of population is one of China's major problems. There is no additional arable land, emigration is impossible, and industrialization is uncertain.

Detailed statistics support all statements. Field work involved filling out elaborate farm and personal questionnaires, and these have been tabulated in a variety of ways. The specialist will find a large amount of detail concerning all aspects of rural life. As examples, five tables at random cover the number and area of graves in farms; the percentage of farmers who are owners, part owners, and tenants; the chemical composition, calorific value, and vitamin value of foods consumed; the number and kind of livestock per farms; and the number of rooms, doors, and windows per capita.

Volume One presents a broad picture of the land and its problems, with chapters on agriculture and its regionalization, topography, climate, soils, crops, livestock, the farm business, marketing and prices, population, and standards of living. Volume Two is the Atlas, with 170 maps which present the statistical material tabulated in Volume Three. Over 300 tables provide a wealth of specialized data for further analysis.

Land Utilization in China is a work of fundamental importance, both as

an interpretation of China and as an example of procedure in agricultural economics. Until adequate census data are someday available, this collection of sample studies will be the standard source of statistical information on China. Dr. Buck and his colleagues have accomplished an exceedingly difficult task with unusual success.

GEORGE B. CRESSEY

Syracuse University

Part-Time Farming in the United States, Prepared under the supervision of Z R. Pettet, Chief Statistician for Agriculture. Washington, D. C.: Bureau of the Census, United States Department of Commerce, U. S. Census of Agriculture: 1935. 1937. 205 pp. 50 cents.

In this publication the Census Bureau presents data collected in the Census of 1930 and of 1935 relating to part-time farming in the United States. Much of the data included are new and are in addition to the data presented relating to part time farming in the regular volumes of the 1930 and the 1935 Census reports. The Bureau states that one of the primary purposes underlying the study was to delimit the areas where part-time farming occurs.

New data presented include the number of days worked for pay or income not connected with the operator's farm in 1929 and a further breakdown of the number of days worked off the farm in 1934. Part-time farmers for 1934 are classified by color and by tenure status. Tables 7, 8, and 9 present data from the 1930 Census relating to the nature of the off-farm work. It is interesting to note that, for the United States as a whole, the rural farm male population having nonagricultural pursuits represented only 3.5 per cent of all males having nonagricultural pursuits. For forestry and fishing it was 16.9 per cent, for mining 7.2 per cent, and for automobile factories and metal industries 2 per cent. One-half the volume is devoted to an analysis of the organization of the so-called part-time farms as contrasted with all farms as a group in 13 widely scattered counties and in the Columbus, Ohio, metropolitan area. It is stated that the Columbus area was selected to study the organization of part-time and non-part-time farmers in a thickly populated locality and the organization of the same type of farms in a locality less thickly populated when the two areas offered the same general opportunities to farm operators for outside work.

The volume makes available much new data relating to the nature of part-time farming in the United States. One must keep in mind, however, the Census definition of a farm which for 1929 and 1934 did not include tracts of land of less than three acres unless its agricutural products for the year were valued at $250 or more; furthermore, that to have been classed as a part-time farm the operator must have worked off the farm for pay. Some other member of the family working off the farm for pay would not make the farm part-time. It is the opinion of the reviewer that too much reliance should not be placed upon a comparison of the 1929 and 1934 data,

owing to the probable inadequate reporting of small farms by the Census of 1930.

It is to be hoped that the Bureau of the Census may find it possible to make a somewhat similar study of the characteristics and occupations of non-farm rural residents.

JOHN I. FALCONER

Ohio State University

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* Beginning with this issue, the Journal will carry a list of publications received. Items that are not of interest to statisticians will be omitted from the list. The contents of periodical publications will not be listed but the attention of the reader is directed to the lists of articles in current publications which are to be found in the *Revue de l'Institut International de Statistique*, *Journal of the Royal Statistical Society*, *American Economic Review*, *Population Index*, *Transactions of the Actuarial Society of America*, *The Record of the American Institute of Actuaries* and *Sankhyā, The Indian Journal of Statistics*.—Editor.

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